# THE JOURNAL
OF THE
# ROYAL AERONAUTICAL SOCIETY

(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Fellow

No. 196. APRIL, 1927. Vol. XXXI.


## NOTICES

### *Annual General Meeting*

The attention of all members is particularly called to the report of the Annual General Meeting on pages 271-272. A statement of the effects of the Beharrell report will be circulated in the course of the next few days to all members, so that the opinion of the general body of voters may be obtained. It is the expressed wish of the Council that every member will make a special effort to reply as soon as possible to any points which may be raised in the statement, so that the Council will have a guide as to their future action.

### *Elections*

The following Members were elected at a Council Meeting held on March 9th:—

*Associate Fellow.*—Major F. C. Buck.
*Associate.*—Mr. T. M. Naylor, M.Sc., A.M.Inst.C.E., A.M.I.Mech.E.
*Students.*—Mr. A. Wallace, Mr. S. C. Ilett.
*Members.*—Mr. H. Beamish, Mr. L. W. Dent.

### *Donations Received*

Many members have asked how the amount in the Income and Expenditure Account, "Donations Received £508 10s. 0d.," is made up. The principal donations are as follows:—

| | |
|---|---|
| Air Ministry (annual grant) ... ... ... | £250 |
| S.B.A.C. ... ... ... ... ... ... | £250 |

The Air Ministry grant is an annual one for five years.

### *Branch Notes*

*Westland Aircraft Society.—Yeovil Branch of the Royal Aeronautical Society.*—The membership of the Yeovil Branch has now increased to 130.

*Branch at Leeds.*—A branch of the Society has recently been formed at Leeds with Professor W. T. David, M.A. (Cantab), Sc.D. (Cantab), D.Sc. (Wales), M.Inst.C.E., M.I.Mech.E., Professor of Engineering at Leeds University, as Chairman, and Mr. T. M. Naylor, M.Sc., A.M.Inst.C.E., A.M.I.Mech.E., Associate, as Honorary Secretary.

### *Elliott Memorial Prize*

The subject of the first award of the Elliott Memorial Prize, to be competed for by the boys being trained at Halton, is "The Ideal British Aircraft Mechanic, with particular reference to the work of the late A. B. Elliott." This prize was founded by the Society as a memorial to the late Mr. A. B. Elliott, who contributed so greatly to the success of Sir Alan Cobham's flights to India, South Africa and Australia.

### *Royal Air Force Display*

The Committee of the Royal Air Force Display state that the Annual Display will take place at Hendon Aerodrome on Saturday, July 2nd, 1927, between the hours of noon and 6.30 p.m.

### *Informal Discussion*

An informal discussion on "Light Planes," to be opened by Mr. W. E. Gray, will take place in the Society's Library at 7.0 p.m. on Friday, April 8th.

The discussion has been arranged by the Students' Section, but members in any grade will be welcome.

### *Students' Section*

On Saturday, February 26th, a party of about 60 students, including a group from the City and Guild Engineering College, visited the De Havilland Aircraft Co. at Stag Lane Aerodrome. They were shown all over the workshops and took great interest in the mass production of "Moths" and in "Hercules V." which was nearing completion. Afterwards a number had flights in "Moth" machines.

On Friday, March 18th, a very interesting lecture on the "Recent Developments in the Construction of Rigid Airships" was given by Mr. W. T. Sandford, of the Airship Guarantee Company, with Major Scott, A.F.C., C.B.E., in the chair. Mr. Sandford gave a clear account of problems met with in the construction of rigids and showed how they might be overcome. Seventeen students were present and about twelve took part in the discussion.

### *Library*

The following books have been recently received and placed in the Library:—Reprints of two papers by E. H. Hankin, "The Evolution of Flying Animals" and "Soaring Flight"; "Kolben fur Kraftfahrzeugmotoren: Grauguss, Aluminium, Elektron," by E. Mahle; "Der Luftschiffbau Schutte-Lanz 1909-1925," by J. Schütte; R. & M. No. 1041, "Accidents to Aeroplanes Involving Flutter of the Wings"; R. & M. No. 1045, "On the Equivalence between the Dynamical System of a Multi-Crank Flywheel System and a certain Electrical Circuit . . .," by E. B. Moullin; "Diesel Engines," by A. H. Goldingham; R. & M. No. 1049, "Direct Measurement of the Angle of Flight Path, . . .," by E. T. Jones and H. L. Stevens; R. & M. No. 1051, "Second Report on Full-Scales Experience with the Slot and Aileron Control, . . .," by R. L. Stevens; "Fogs and Clouds," by W. J. Humphries; R. & M. No. 1034, "The Efficiency of an Airscrew," by H. Glauert; "Turbomotore a Compressore a Nafta," by Gen. A. Guidoni; Report on Civil Aviation, Department of National Defence, Ottawa; "The Study of War," by Sir George Aston; "Mechanics Applied to Engineering," Volumes I. and II., by J. Goodman; and "Land, Sea and Air," by Admiral Mark Kerr.

### *Forthcoming Arrangements*

Thursday, April 7th, **5.15 p.m.**—At the Royal Society of Arts, 18, John Street, Adelphi, W.C.2, "Parachutes," by Flight Lieut. F. O. Soden, D.F.C.

Thursday, April 28th, at 6.30 p.m.—At the Royal Society of Arts, 18, John Street, Adelphi, W.C.2, "Seaplane Development," by Major R. E. Penny, A.F.R.Ae.S.

J. LAURENCE PRITCHARD, *Secretary.*

## *OBITUARY*

FLIGHT LIEUTENANT W. A. MEGGITT, *Associate.*

It is with great regret we have to record the death, through the crashing of his aeroplane at Croydon on January 28th last, of Flight Lieutenant W. G. Meggitt, an Associate of the Society. Flight Lieutenant W. G. Meggitt joined the Society on January 11th, 1927, and was one of its most enthusiastic members.

The Council wish to express their deep sympathy with Mrs. Meggitt on the loss of her husband.

# ANNUAL GENERAL MEETING

The Sixty-Second Annual General Meeting of members of the Society was held in the Society's Offices, 7, Albemarle Street, W.1, at 5.0 p.m., on Tuesday, March 29th, 1927, Colonel the Master of Sempill, Chairman of the Society, presiding.

Mr. W. P. Savage, Mr. G. H. Dowty and Mr. P. Narraway were appointed scrutineers of the ballot for the election of new members to the Council. As a result of the ballot the following members were declared duly elected to serve on the Council for the two years ending March, 1929:—Wing Commander T. R. Cave-Browne-Cave, Sir Mackenzie Chalmers, Mr. A. E. L. Chorlton, Mr. C. R. Fairey, Mr. J. E. Hodgson, Major R. H. Mayo, Colonel Mervyn O'Gorman, Mr. T. O. M. Sopwith, Sir Vyell Vyvyan and Dr. H. C. Watts.

The minutes of the last Annual General Meeting were taken as read and confirmed.

The Council's report for the year 1926-1927 and the balance sheet and accounts, as published in the March issue of the Journal, were adopted.

Rule 18, which read:—

> "The President must be a Voter and shall be appointed by the Council. He shall be an *ex officio* member of the Council and of all Committees appointed by the Council, and shall be entitled to preside at all Meetings at which he shall be present,"

was amended to read as follows:—

> "The President must be a Voter and shall be appointed by the Council. He shall take office for one year, but shall be eligible for re-election for a further period of one year after his first year of office. If elected for a second year he shall not be eligible again for further election until two years have elapsed since the expiry of his second year of office, when he shall be eligible. He shall be an *ex officio* member of the Council and of all Committees appointed by the Council."

Rule 19 (a) which read:—

> "At a Meeting held in June in each year, the Council shall appoint a Chairman, who shall take office in the following October, and hold office for one year. He shall be an *ex officio* member of the Council and of all Committees appointed by the Council, and shall preside at all Meetings at which he shall be present in the absence of the President. He shall not be eligible for re-election on the expiry of his year of office,"

was amended to read as follows:—

> "At a meeting held in June in each year, the Council shall appoint a Chairman, who shall take office in the following October, and hold office for one year. He shall be an *ex officio* member of the Council and of all Committees appointed by the Council. He shall be eligible for re-election on the expiry of his year of office for one further year, but, if elected for a second year, he shall not be eligible again for further election until two years have elapsed since the expiry of his second year of office, when he shall be eligible,"

and Rule 19 (paragraph c) which read:—

> "The Vice-Chairman of the Society shall be the retiring Chairman. He shall be an *ex officio* member of the Council, and shall hold office for one year. He shall preside at all Meetings at which he shall be present in the absence of the President and Chairman,"

shall be amended to read as follows:—

> "The Vice-Chairman of the Council shall be the retiring Chairman. He shall be an *ex officio* member of the Council and of all Committees appointed by the Council, and shall hold office for one year. In the case of a Chairman being elected for two years in succession the Council may ask the existing Vice-Chairman to continue his office for a second year, or may elect a member of the Council to this office for the same period."

The following resolution, Item V. of the Agenda, was proposed by Mr. H. T. Tizard and seconded by Mr. J. D. North:—

> "That this meeting supports the action of the Council in endeavouring to come to an agreement with the Institution of Aeronautical Engineers on the lines discussed in the Beharrell report, and authorises the Council, if satisfied that the general body of the voters of this Society and of the Institution of Aeronautical Engineers endorses their views, to carry the amalgamation into effect with such alteration to the draft terms of the agreement that appear to them desirable."

On a show of hands the resolution was declared carried by a large majority.

A vote of thanks was proposed and seconded to the scrutineers for their services and to the Chairman and the meeting closed.

# LETTERS OF AVIATION PIONEERS

[The Society has recently acquired four interesting letters—one from the late Mr. Wilbur Wright, two from Wilbur and Orville Wright, and one from the late Professor S. P. Langley. As it is thought these letters will be of interest to members, copies of them are printed below. The letter from the late Mr. Wilbur Wright is particularly interesting both for its brief account of the work of himself and his brother and for the reference to Professor Langley. All the Wright letters are interesting in the way they throw a light on the characters of the two brothers, showing their willingness to give information of their work.—EDITOR.]

COPY.

(*Estabd.* 1892) "*Wright Cycle Co.,*" 1127, *West 3rd St., Dayton, Ohio.*

Mr. Sidney H. Hollands. July 16th, 1905.

Dear Sir,

We have received your letter with much pleasure.

Our experiments were continued during a considerable part of the summer and autumn of last year with quite favourable results.

It is not our desire, however, that too much interest in our work should be publicly aroused at present, and accordingly cannot make any extended report until the experimental period is past. In 1904 we succeeded for the first time in making complete circles, and landing at the starting point.

On two different occasions the flight extended over nearly four rounds of the field, a distance of nearly three miles. The duration of the flight was five minutes and four seconds in one case, and four minutes and fifty seconds in the other. The speed was about 35 miles per hour. The new problems—previously unsuspected—which are met at almost every step consume much time in developing a machine for practical use, but the critical points have been passed, and within a reasonable time we expect to have the machine ready to present to the world for some serviceable purpose.

The articles in *Flying* were read at the time with interest, and it would please us to see some experiments along that line, to see whether it would offer any advantages over gliding as a method of practice. We hear from Mr. Chanute now and then, and at last report he was well, though he had a troublesome attack of the influenza last winter.

You have probably seen some account of the experiments which Mr. Avery made with a Chanute glider at St. Louis last summer. We have no recent word regarding Prof. Langley, but think he has abandoned the effort to fly his machine. It was evidently too frail to be of real value for experimental purposes. There is much more interest taken in aviation than for some years past, and, except the experiments of Mr. Montgomery, little of the work is of much value.

Yours truly,

(Signed) WILBUR WRIGHT.

COPY.

Wilbur Wright.
Orville Wright.

*"The Wright Cycle Company" (Estabd. 1892), 1127, West 3rd Street, Dayton, Ohio.*

Mr. Sidney H. Hollands,
Sheffield, England. May 31st, 1904.

Dear Sir,

Your letter to us, sent through Mr. Chanute, was received some time ago, and would have been promptly answered but for the fact that we were expecting almost daily to be able to send you some account of more trials of our flyer. However, we have already been so long delayed that we shall not wait longer in answering. We expect to be able to make a trial of our new flyer within the next ten days. It is almost an exact copy of the one we used at "Kitty Hawk, N.C.," last December, but it is so constructed that it can be more easily torn down for shipment. We have also slightly changed the shape of the surfaces for the purpose of higher efficiency at high speeds. This, of course, will somewhat increase the difficulties in getting started, on account of the higher initial velocity required, but we do not anticipate any serious trouble on this account. Your letter of last year, of which you write, asking for photographs, was not received. We are not yet ready to make public the details of our flyer, and so have been compelled to refuse all requests for photographs, but we will be pleased to furnish you information from time to time of our progress.

Thanking you for your most welcome letter,

We remain,

Respectfully yours,

(Signed) W. & O. WRIGHT.

COPY.

Wilbur Wright.

Orville Wright.

*Wright Brothers, 1127, West 3rd Street, Dayton, Ohio.*

Mr. Sidney H. Hollands,
Sheffield, England. January 31st, 1907.

Dear Sir,

Your letter of November 24th was duly received. We were pleased to hear from you again.

We have made no further outdoor experiments since October, 1905, and consequently have nothing very interesting to report at the present time. We have been devoting all our time to the building of new machines, and to business matters. The propellers we have been using give us such good results that it is not probable that it would pay us to look for anything better. What we most need now is a perfectly reliable motor that is able to run continuously for hours at a time. We think that our new motors will give much better results than the one we used in our last flights.

Our new engine weighs 160 lbs., not including water, etc.; it will weigh about 250 lbs. It will produce from 25 to 30 horse-power. This is only about two-thirds the weight per horse-power of our old motors.

If we find we can make it more reliable by building it heavier, we shall not hesitate to do so, for reliability is of a great deal more importance than lightness.

Very truly yours,

(Signed) WRIGHT BROTHERS,
per O.W.

COPY.

"Smithsonian Institution,"
Washington.
January 27th, 1903.

Dear Sir,

I have your letter of January 1st, in which you ask for information regarding the work in aerodromics which I am now conducting. I am not at present prepared to make public any results that I have obtained, further than what has already been published, and I therefore regret that I cannot comply with your request.

I cannot speak for Mr. Bell regarding his work, since whatever he has done has been entirely independent of my own.

In compliance with your request, I take pleasure in sending you, under separate cover, my photograph, and beg to remain

Very truly yours,

(Signed) S. P. LANGLEY, *Secretary.*

Sidney H. Hollands, Esquire,
Firth Park, Sheffield.

## BANQUET TO SIR SAMUEL HOARE SECRETARY OF STATE FOR AIR

A banquet was given to Sir Samuel Hoare, Secretary of State for Air, at the Savoy Hotel on Wednesday, March 2nd, 1927. The banquet, given under the joint auspices of the Royal Aeronautical Society, the Royal Aero Club, the Society of British Aircraft Constructors, and the Air League of the British Empire, was to commemorate the recent flight to India and back by Sir Samuel and Lady Maud Hoare.

Lord Thomson, Chairman of the Royal Aero Club, presided over a brilliant gathering. In proposing the toast of "Our Guest," Lord Thomson prophesied that in ten years' time no business man would ever think of travelling between London and the East by any route other than the air, and he expressed his belief that Sir Samuel and Lady Hoare were the forerunners of innumerable distinguished British couples who would prefer to travel by air to India and the most remote parts of the world.

Sir Samuel Hoare, replying to the toast of his health, said that when he came to introduce his air estimates in the House of Commons there would be very little to which Lord Thomson and his colleagues could object. In fact, during the years since the war there had been a continuity in the air policy of this country. The part he played in the journey to India and back was not in any degree comparable with the work of the great pioneers. The flight was admirably organised by the Imperial Airways Company, and throughout there was the closest co-operation between the civil and Air Force authorities. At a Persian landing ground a man in a fez wished to talk to him, and he sent for a Persian interpreter, but in perfect English the man said: "I am a member of the Manchester Light Aeroplane Club, and I am travelling in Manchester goods." What better instance, added Sir Samuel, could there be of the growing tentacles of the light aeroplane clubs that had been started?

In India the people took the greatest interest in the machine. At Delhi he arranged for a flight for the members of the Viceroy's Council, and when he had packed them in, someone came to him and said, "Could you not arrange for a crash?" According to one Continental expert there was one deficiency in the tour, for that expert said to him, "Supposing at the end of your journey you had been killed in a railway accident, it would have put the coping-stone on an otherwise altogether successful tour." He could not help thinking that the journey would have a very definite effect on civil flying. Perhaps not as a consequence, but as an interesting sequel, the Government of India had decided to embark on a policy of civil aviation. It would, he was sure, expedite the time when we would have regularly organised Empire air routes. In one respect especially the flight had done good, for when the machine arrived on the tick of time people regarded it as the outward and visible sign of British enterprise and British efficiency.

The health of the Chairman was proposed by Viscount Haldane, the Right Hon. L. S. Amery, M.P., Secretary of State for the Dominions and Colonies, seconding.

Amongst those present were the Right Hon. W. C. Bridgeman, M.P. (First Lord of the Admiralty), Sir Philip Sassoon, M.P. (Under-Secretary for Air), Marshal of the Royal Air Force Sir Hugh M. Trenchard, Air Vice-Marshal Sir Vyell Vyvyan and Lady Vyvyan, Air Vice-Marshal Sir John F. A. Higgins, Lord Gorell, the Master of Sempill (Chairman of the Royal Aeronautical Society), the Hon. Mrs. Forbes-Sempill, Sir Francis K. McClean (Vice-Chairman of the Royal Aero Club) and Lady McClean, Sir Herbert Hambling, Sir Samuel and Lady Instone, Mr. T. O. M. Sopwith (President of the Society of British Aircraft Constructors) and the Hon. Mrs. Sopwith, Mr. H. T. Vane (Vice-President of the Society of British Aircraft Constructors) and Mrs. Vane, and Mr. Philip S. Foster (Chairman of the Air League of the British Empire).

# PROCEEDINGS

## SEVENTH MEETING, SECOND HALF, 62ND SESSION

The Seventh Meeting of the Sixty-second Session of the Royal Aeronautical Society was held in the Theatre of the Royal Society of Arts, 18, John Street, Adelphi, W.C.2, on Thursday, January 6th, 1927, when Major B. C. Carter, A.R.C.Sc., D.I.C., Associate Fellow, read his paper on "Dynamic Forces in Aircraft Engines." Colonel the Master of Sempill, Chairman of the Society, was in the chair.

The CHAIRMAN said: Before introducing the Lecturer this evening I am sure all members of the Society will wish to congratulate one of our past chairmen, Colonel Tizard, on the very well deserved honour recently conferred upon him by H.M. the King.

As many of you know, Air Vice-Marshal Sir Geoffrey Salmond has just relinquished the post of Air Member for Supply and Research and is now on his way to India in one of the aeroplanes destined to operate on the Cairo-Karachi route. We shall all wish Sir Geoffrey success as the A.O.C., India, and we should like to record, not only our great appreciation of all he does for the Society, but our admiration of the way in which he administers the technical affairs of the Royal Air Force. At the same time we would offer to his successor, Air Vice-Marshal Sir John Higgins, all the support that the Society is able to give and in addition our very sincere wishes for a highly successful term of office.

The Lecturer is well known to you and needs no introduction from me. I might, however, recall to your memory the fact that Major Carter was one of the first officers to join the Technical Section of the R.N.A.S. at the very commencement of the war. He was one of the mainstays of that department and was largely responsible for the development of many engines that have become famous and are or were in use throughout the world. Since the formation of the R.A.F. Major Carter has continued, with many others from his old department, to serve on the technical side of that Service and it would be no exaggeration to say that he is one of the most brilliant members of the section dealing with engines.

The Royal Aeronautical Society have been hoping that he would be able to lecture on a subject to which he has devoted a great deal of attention, to wit, that of Compression Ignition Engines. The Society has been looking forward to the possibility of having such a lecture for some years, and it is extremely unfortunate that the Air Ministry were unable to give permission for the lifting of the veil of secrecy which surrounds this highly interesting subject. Major Carter had prepared an admirable Paper, but when permission was refused for it to be given he, with characteristic energy and at extremely short notice, set to work to prepare the Paper which I will now ask him to read.

I would like to say that he was one of the very first to study closely this all-important subject of torsional vibration. These studies were commenced during the war, following on the many serious troubles which were being experienced with a certain 8-cylinder water-cooled engine which was then being produced in very large quantities.

# DYNAMIC FORCES IN AIRCRAFT ENGINES

## (TORSIONAL VIBRATION)

BY MAJOR B. C. CARTER, ASSOCIATE FELLOW.

### *Introduction*

Having honoured me with an invitation to deliver a lecture this Session, the Society intimated that the most acceptable subject would be "Heavy-Oil Engines for Aircraft." Unfortunately difficulties intervened which prevented the Society's choice from being adopted, and the outcome has been the hurried preparation of this lecture upon a subject which may seem remote from that mentioned.

As regards the broad title, this remoteness is more apparent than real, however, since the difficulties to be surmounted in evolving heavy-oil engines for aircraft relate quite as much to the mechanics of engine design as to problems of fuel injection and combustion. That this is so may be gathered from the paper Mr. Pye read before the British Association last summer, and from Mr. Chorlton's comments in the December issue of the Society's Journal. In Mr. Pye's paper, information is given which shows that much progress has been made in this country (and notably at the Royal Aircraft Establishment) in the direction of obtaining satisfactory combustion at high engine speeds with airless fuel injection. Also, attention is drawn to the fact that the ratio of maximum to mean pressure must of necessity be appreciably greater with compression ignition than with electric ignition, owing to the greater expansion ratio, so that the compression ignition engine is handicapped on this account in the matter of power for weight.

Mr. Chorlton, in the comments referred to, mentioned that the main difficulties he had experienced had been concerned with the mechanical parts of the engine, notably in the crankcase and in the crankshaft.

Possibly, in course of time, the handicap of high maximum pressure will be circumvented or minimised by the adoption of the two-stroke cycle in conjunction with cylinder arrangements which enable high gas pressures to be neutralised to a large extent by inertia forces. However this may be, there is the more immediate problem of determining the very best use that can be made of four-stroke cylinders having the performance already attained. To produce a satisfactory engine having a few large cylinders presents a design problem involving, *inter alia*, the consideration of dynamic forces, and in particular, the study of torsional resonance; for, in striving after the lightest engine design, no avoidable loads can be allowed to come upon the crankshaft and bearings. By giving thought in the design stage, torsional vibration troubles may be obviated in many if not all cases, a point it is desired to emphasise strongly. The title chosen for this lecture has proved to be too wide, for in attempting to do justice to the subject of torsional vibration I have relinquished the idea of covering other matters.

## PART I.

## TORSIONAL VIBRATION

### *Natural Torsional Vibration*

If a disc of polar inertia I lb. ft.$^2$ be attached to a light wire or rod of stiffness C lb. ft. per radian, as indicated in Fig. (1) the upper end of the wire being fixed to a support of large mass, the frequency of natural torsional vibration of the disc relative to the massive support can be shown to be

$$(1/2\pi)\sqrt{(Cg/I)} \text{ per second.}$$

In the same way, if $I_c$ denote the virtual moment of inertia of the crank system of a single-throw radial engine, and $C_s$ the shaft stiffness from the airscrew to the crankpin centre, the natural frequency of the crank masses in torsional vibration relative to a comparatively massive airscrew is

$$(1/2\pi)\sqrt{(C_s g/I_c)} \text{ per second}$$

and it can be shown that this frequency is the same when the vibration is superimposed upon a uniform rotation of the shaft. To take account of the fact that the airscrew inertia is finite and equal to $I_a$ (say), a modified formula must be used, namely:

$$\text{Frequency} = (1/2\pi)\sqrt{\{C_s g (1/I_c + 1/I_a)\}} \text{ per second,}$$

which gives results very near to those obtained by taking $I_a$ to be infinite.

The problem of finding the natural frequency thus resolves itself into that of determining the values of $C_s$, $I_c$, and $I_a$. Before considering this matter more fully, attention will be turned to the phenomenon of torsional resonance and its bearing upon crankshaft stresses.

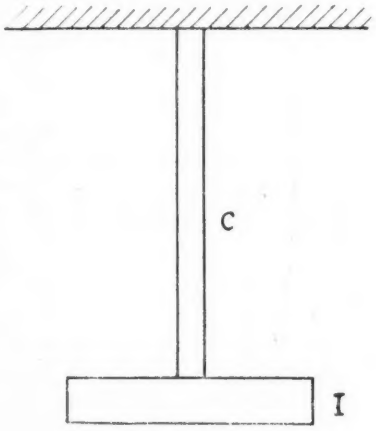



FIG. 1.

## *Forced Torsional Vibration*

Reverting to the system of Fig. (1), consider the case where torsional vibration of the disc is forced by means of an electro magnet through which an alternating current flows, of zero average value, and such as to produce an harmonically varying torque of maximum value B lb. ft. corresponding to a static twist of $E$ radian in the torsion rod, where $E = B/C$.

Clearly, if the disc had no inertia, it would twist backwards and forwards with angular amplitude $E$, and the torque amplitude in the torsion rod would be $B$, the variations occurring in phase with the applied torque. Actually, owing to the inertia of the disc, the amplitude of oscillation *settles down* to some value which will be denoted by $|\theta|$ radian, and the motion lags behind the applied torque, but has the same frequency. The amplitude of torque variation in the rod is then $|\theta|/E$ times $B$, where $|\theta|/E$ may be termed the "Resonance Factor."

In the absence of damping, this factor depends upon one thing only, namely, the ratio of the frequency of the forcing torque to the natural frequency of the system. The connection is expressed by the simple equation

$$\text{Resonance factor} = |\theta|/E = \pm 1/(1 - x^2)$$

where $x$ is the frequency ratio and that sign is taken which gives a positive result. When plotted, this gives Curve 1 of Fig. 2, which has applications to all manner of problems in Science and Engineering where vibrations are concerned, whether such vibrations be material or electro-magnetic in nature. The form of this curve is thus of fundamental interest. It will be noted that $|\theta|/E$ increases from unity to infinity, as $x$ increases from zero to unity; and then decreases from infinity to zero asymptotically as $x$ increases from unity to infinity. The significance of $x$ being unity is, of course, that the vibrations are being forced at a frequency equal to the natural frequency of the system.

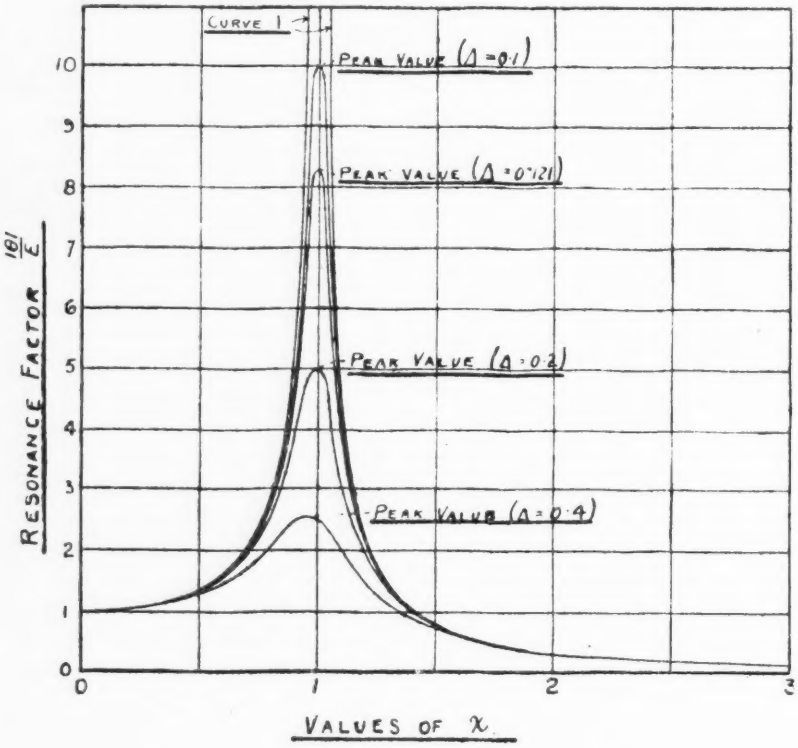



FIG. 2.

There is some damping in every mechanical system, however, and it needs to be considered here. If the damping torque in the present case be taken to be proportional to the vibration velocity of the disc at every instant, the following relation may be established:

$$\text{Resonance factor} = |\theta|/E = 1/\sqrt{\{(1-x^2)^2 + \Delta^2 x^2\}}$$

where $\Delta$ is a damping coefficient and is, in fact, $2/\pi$ times the "Logarithmic Decrement" for natural vibration of the system.

It will be noted that when $\Delta = 0$, this equation becomes the same as the preceding one, and that when $x = 1$ the resonance factor is $1/\Delta$: thus if $\Delta = \frac{1}{5}$, the resonance effect at the condition of synchronism is to amplify the

stress variation in the torsion rod five times. Referring again to Fig. (2), the relation between $|\theta|/E$ and $x$ is seen plotted for various values of $\Delta$.

Several important points are revealed by these curves :—

(*a*) That when $\Delta=0$, resonance factor is greater than unity for all values of $x$ between unity and $\sqrt{2}$.

(*b*) That the degree of damping makes little difference to the curves outside the region of $x=1$, unless $\Delta$ is comparatively large, say, greater than 0.2.

(*c*) That if $\Delta$ is not greater than 0.2, the resonance factor is about 2 when $x=0.7$ and 1.2, and is greater than this within these limits.

(*d*) That when $x$ is greater than $\sqrt{2}$, the effect of resonance is beneficial, *i.e.*, torsion rod is less stressed than it would be had the disc no inertia.

### *Application to the Single-Throw Radial Engine*

Consider now a single-throw radial engine, complete with airscrew, running under its own power. The inertia forces of the main moving parts have a resultant which is practically constant and which acts outwards along the crank direction within such narrow limits of angle that the inertia torque is small compared with the torque produced by the cylinder pressures at full throttle. What inertia torque there may be is the result of having a master and articulated rod system, and the frequency of its variation is the same as that of the crankshaft rotation, whereas, with, say, nine cylinders, the frequency of the explosion impulses for four-stroke operation is 4.5 times this, so that the frequency per second is r.p.m./13.3. In what follows the inertia torque will be disregarded.

There is shown in Fig. (3) a torque curve calculated for a nine-cylinder engine with even firing, and it will be seen that to a first approximation this may be regarded as comprising a constant torque of 1,280 lb. ft. upon which is superimposed a simple harmonic variation of $\pm 520$ lb. ft. The amplitude of the variation is thus about 40 per cent. of the mean. Owing to its large inertia the airscrew rotates at almost uniform speed, notwithstanding this torque variation, and the crankshaft, acting as a torsion spring, has an average twist corresponding to the mean torque, but it winds and unwinds to the extent represented by the actual torque variations in it. As in the analogous case of the torsional pendulum, if the crank system had no inertia, the shaft twist would follow the gas torque variations instantly, and the shaft stresses would correspond to these variations. Actually, owing to the inertia of the crank system, the gas torque variations need to be multiplied by the appropriate " Resonance Factor " to arrive at the torque variations in the shaft, and these variations lag behind those of the gas torque, but occur with the same frequency. At the main syncronous speed this frequency is equal to the natural frequency of the system and the " Resonance Factor " is equal to the reciprocal of the damping coefficient $\Delta$.

So far as I have been able to determine, the value of $\Delta$ for an engine of the type under consideration is in the region of 0.2 to 0.1, and thus the torque variation in the shaft at synchronous speed may be from 5 to 10 times the corresponding gas-torque variation. Thus if the frequency of the main explosion impulses becomes equal to the natural frequency of the system at or near full throttle speed it may readily be imagined that crankshaft failure will ensue, either outright, or by fatigue after a period of running in this speed region, determined by the severity of the resonance effect and the shaft strength to resist fatigue loadings—a resistance which, incidentally, is lowered by oil holes, sharp fillets, splines, or keyways, and by rough boring ; also, of course, by wrong heat treatment of the material.

Over the speed range from 30 per cent. below to 20 per cent. above, the speed corresponding to synchronism, the "Resonance Factor" is greater than 2 or thereabouts. Beyond $\sqrt{2}$ times the synchronous speed the "Resonance Factor" is less than unity.

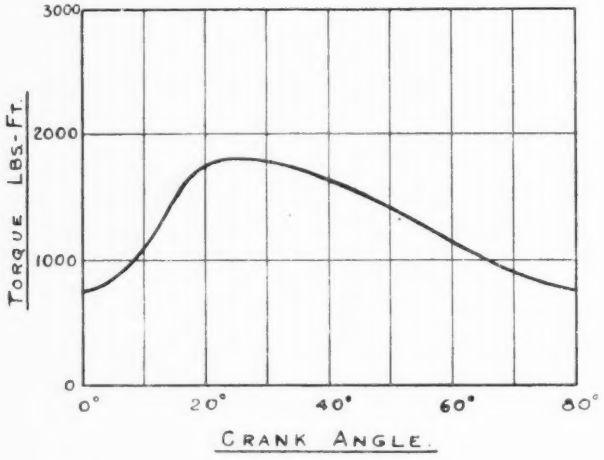



FIG. 3.

It might well be asked at this juncture:—

(*a*) Do resonance effects actually occur in radial engines in conformity with the theory?

(*b*) Can calculations be made with confidence in the design stage that will enable harmful torsional resonance effects to be obviated or minimised?

(*c*) Can torsional resonance be converted from being an enemy into a friend —by suitable design?

I have been impressed with the practical importance of these questions and have endeavoured to obtain convincing answers to them: from what follows I think it will be agreed that in each case the answer is in the affirmative.

In 1923 I was permitted to give to the Society some information respecting work which had been done on this subject at the Air Ministry and at the Royal Aircraft Establishment,* and it is not proposed to go all over the same ground again; suffice it to say in reference to single-throw radial engines, that two important engines of this type proved a failure (after getting well into production), owing to crankshaft troubles which I am quite convinced were attributable to torsional resonance effects. Such a trap for the designer was not to be ignored, nor, in respect of such engines, is it ignored now in this country: on the contrary, the consideration of resonance effects has become practically the starting point of design. Reference to the matter as it affects multi-crank engines will be deferred until further attention has been given to the relatively simple case of single-throw radial engines.

A Hopkinson-Thring type of instrument adapted to reveal cyclical fluctuations of torque was designed for the Air Ministry by Siemens Brothers, Ltd., of Woolwich, to fit into the hollow crankshaft of one of the engines mentioned above, but in a final attempt to save the engine, solid crankshafts were adopted

* Letter in *Journal Royal Aeronautical Society*, Vol. XXVII., November, 1923.

and the instrument was not proceeded with. Later on, the Siemens design was put before the Mechanical Testing Department of the Royal Aircraft Establishment with a request to use it as a basis for evolving an instrument to fit into the crankshaft of another single-throw radial engine (now obsolete) and the outcome has been the instrument shown in Fig. 4, in which, however, the essential features of the Hopkinson-Thring optical system have been discarded in order to obtain a polar diagram on a ground glass screen or photographic plate.

Mr. Douglas and Mr. Gerard of the R.A.E., evolved this torsiograph. It is robust in design and suffers no change of zero after being run up to 1800 r.p.m. in the engine on the hangar. Twist variations in the shaft amounting to only fractions of a degree in magnitude, occurring with a frequency of 135 per second, have been recorded without difficulty.

This instrument will be described in general terms only. One portion screws into the port end of the crankshaft and carries a short pivoted cross member, to the centre of which is fixed a stainless steel concave mirror. Within the hollow shaft is mounted a stiff light tube with a screw and cone device at the inner end whereby this end may be expanded to grip the inside of the shaft near the front crank web. The other end of the tube is supported in a ball bearing and has a short open slot which engages a short rod projecting inwards from the cross member, parallel to the shaft axis at a distance equal to the tube radius. Provision is made for adjusting the mirror direction. It will be understood that any twisting of the shaft over the length spanned by the instrument must cause corresponding tilting of the mirror. A Pointolite lamp is placed in front of the engine, 40 inches from the mirror, beside a camera into which an image of the Pointolite is focussed by the mirror. If the twist in the shaft were constant during rotation, the spot of light would describe a circle, but with varying twist a varying curve is traced; one can in fact observe an outward deflection of the light-spot as the engine is pulled over compression for starting. With the engine running, the wavy curve is distorted through general vibration relative to the Pointolite, and bending of the shaft. To enable corrections to be made for this there is a second concave mirror, fixed to the body of the instrument, and the light-spot from this gives a reference line, roughly circular, relative to which the movements of the other light-spot may be measured. Some diagrams taken at a number of speeds, in opening up the engine to full speed, are reproduced in Figs. 5a, 5b, 5c, 5d, 5e, and in Fig. 6 there is given a calculated polar torque curve for full throttle plotted to the same scale. By comparison it will be seen that resonance effects are very pronounced although the synchronous speed is below full speed and the engine was throttled appreciably at this speed. The diagrams reproduced were taken with the test fan cut down to give 1800 r.p.m. at full throttle. Previously, the test fan gave 1640 r.p.m. at full throttle, and in what follows experiments made before the fan was cut down are described.

A cinematograph film was taken during a run up to speed and measurements were made from photographs on the film. To enable the speed for each picture to be determined three instruments were arranged to come into the same picture as the diagram on the ground glass screen, namely, an integrating speedometer, a seconds clock, and a high speed time indicator. The camera was driven from the engine through a creeping gear so as to obtain successive photographs at steadily changing crank positions.

From separate photographs, and from the film, the maximum and minimum torques have been measured over the speed range; these are shown plotted in Fig. (17). In this figure a parabola has been drawn to the origin, from the point representing the mean torque at full speed, namely, 1640 r.p.m., to give approximately the torque needed to drive the airscrew at every speed in the range. Similar parabolas have been drawn through the points representing maximum and minimum torques at full speed, to indicate roughly the limits within

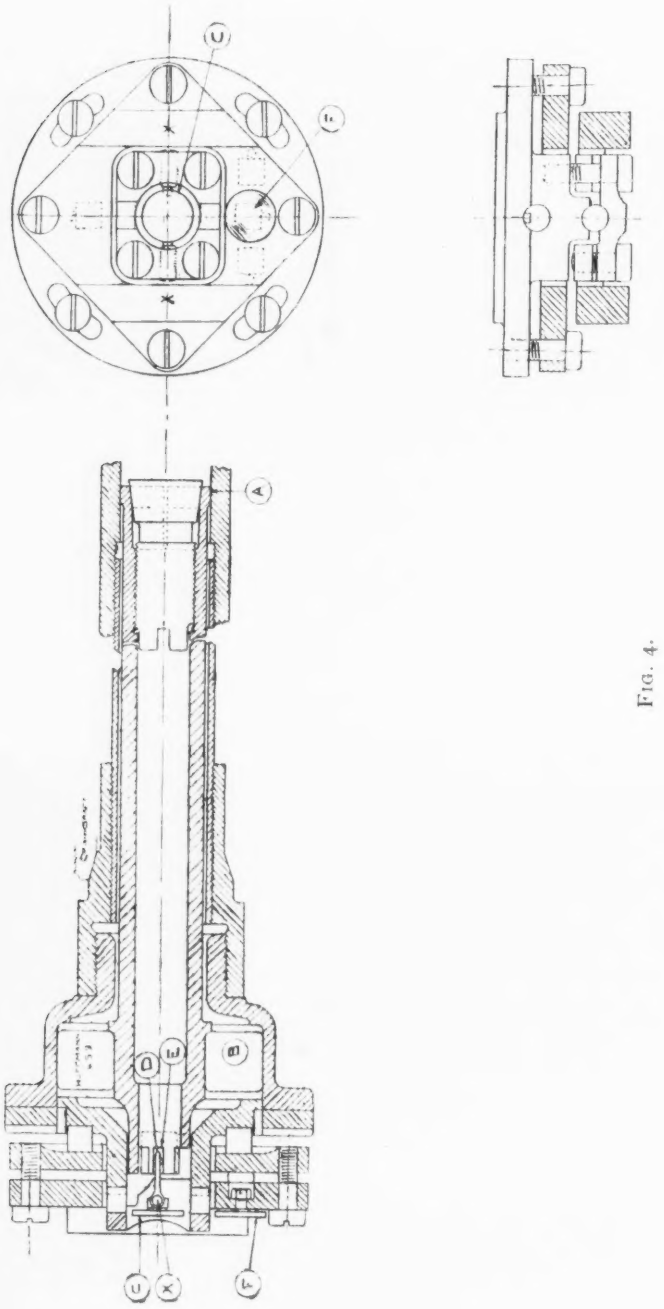
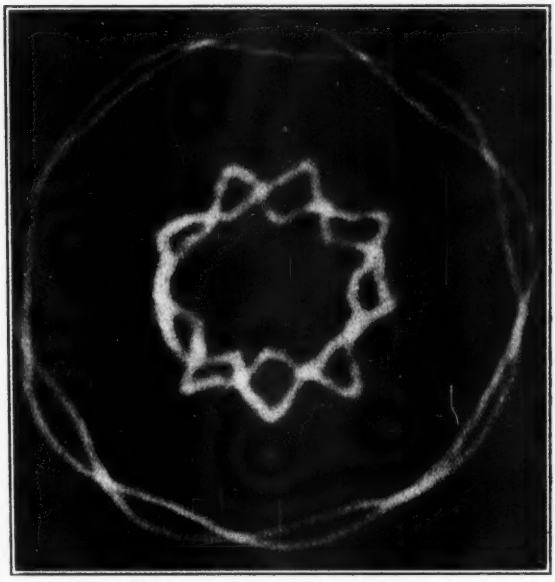
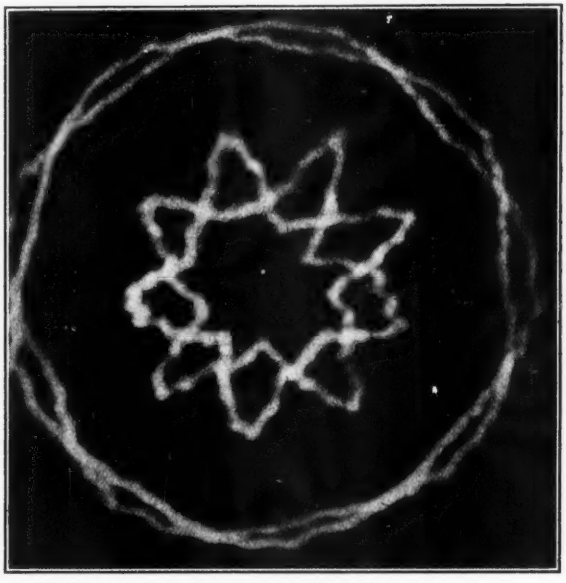


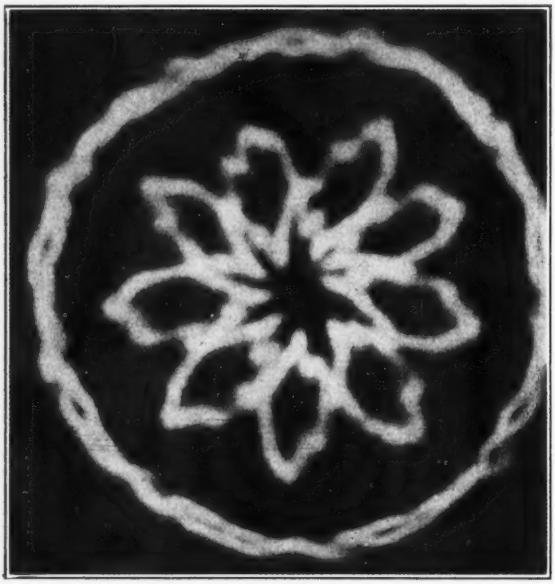
FIG. 4.



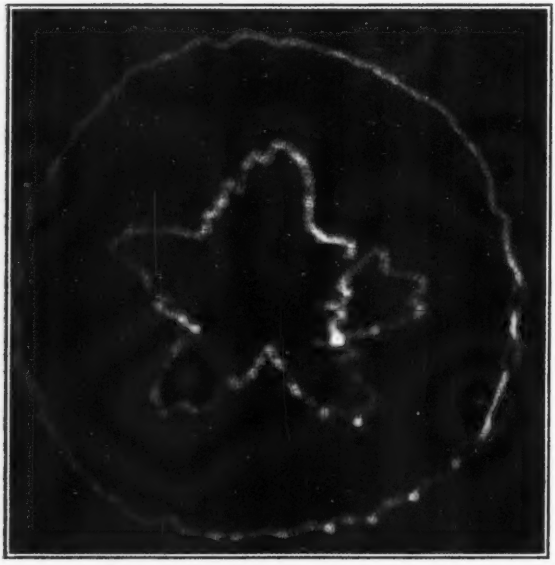
1,400 *r.p.m.*
FIG. 5A.



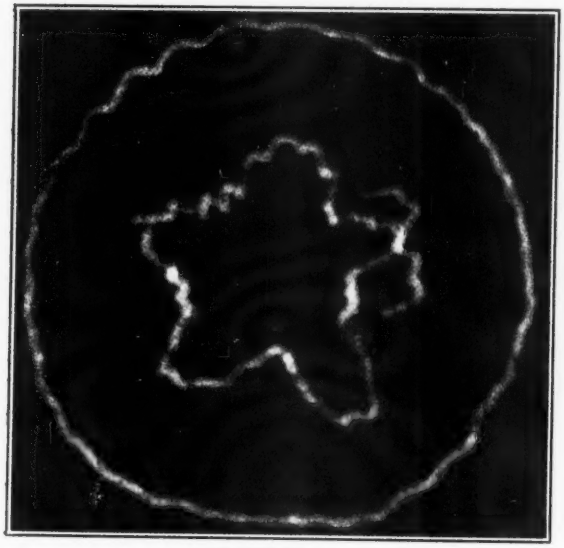
1,500 *r.p.m.*
FIG. 5B.



1,600 *r.p.m.*
FIG. 5C.



1,700 *r.p.m.*
FIG. 5D.



1,800 *r.p.m.*

FIG. 5E.

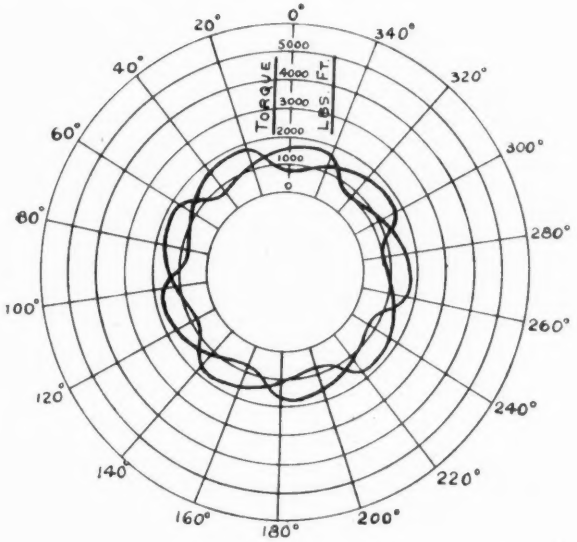



FIG. 6.

which the torque variation would lie at any given speed, were there no resonance effects. The maximum ratio of observed torque variation to the estimated gas torque variation is 8.2 and occurs at 1400 r.p.m. The synchronous speed is thus 1400 r.p.m. or thereabouts, and the coefficient of damping, according to this somewhat rough and ready method of estimating it, is 0.12.*

With the parabolas as a basis, the theoretical torque variation has been calculated for $\Delta=0.12$, and 1400 r.p.m. synchronous speed, and plotted on the same graph. It will be noted that although the theoretical curve agrees well with the observations above 1400 r.p.m., the observed torque variation increases much more steeply than the theoretical curve on the low speed side of the peak.

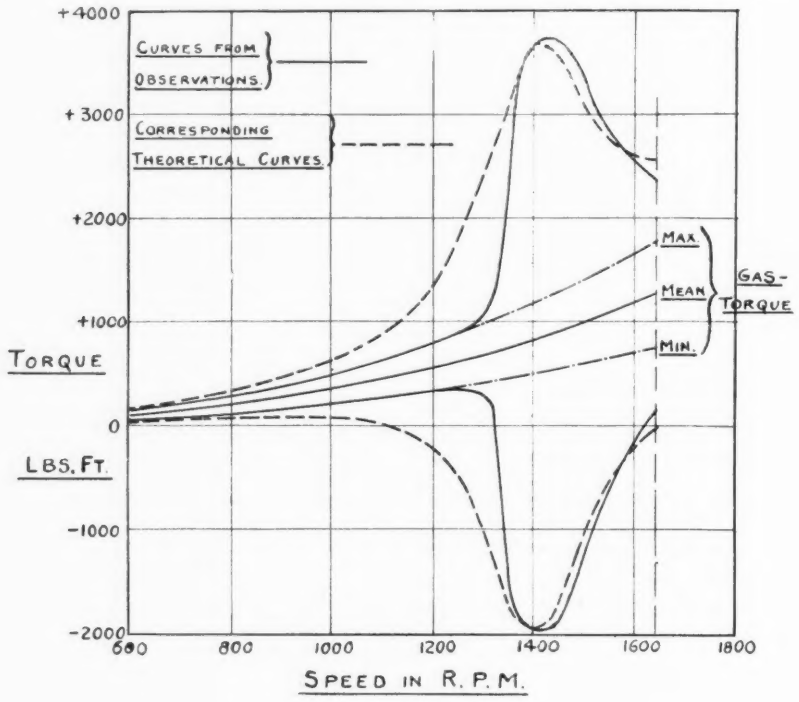



FIG. 7.

This may be attributable to the fact that the measurements were made from a film taken while the engine was run quickly up to speed; moreover, the stiffness of the connection between the airscrew and the shaft varied according to the magnitude of the applied torque, so that the natural frequency of the system was dependent upon the amplitude of movement. Part of this "spring" took place between the hub and the parallel splines on the shaft (the point of drive moving along the splines with changing load, and the hub stretching circumferentially and tending to over-ride the splines), and part took place between the wood airscrew boss and the metal hub, through inadequate

* N.B.—For exact treatment the torque curve at each of several throttle openings and speeds, would need to be known and the fundamental amplitude determined by Fourier analysis.

grip and slewing of the airscrew bolts in taking up a drive not intended to come upon them.

Fig. (9a) shows the results of a stiffness test. This was made some little time after the vibration tests and the fixing was not necessarily the same as during the vibration tests. The area of the hysteresis loop—of which one half is shown—corresponds to 10.4 ft. lb. of energy. The torque reversal is more than sufficient to embrace the loop at 1400 r.p.m. and at this speed the rate of energy dissipation would be equivalent to $10.4 \times 1400 \times 4.5/33000 = 2$ horse power. About three-fourths of this is represented by movement between the wooden boss and the hub. From the known inertias, the shaft stiffness for the synchronous speed to be 1400 r.p.m. has been calculated to be 142,000 lb. ft./rad.

A line of corresponding slope has been drawn in Fig. (9a) through the mid-point of the hysteresis loop. Although the hysteresis prevents exact calculation, the slope of this line is a sufficiently good average to indicate that the method of calculating frequencies is not greatly in error.

In Fig. (9b) the results of stiffness tests made with the hub and test fan nuts tightened are given. The hysteresis loop is greatly reduced and what remains is principally due to movement of the hub on the spline.

The diagrams of Fig. 5a, 5b, etc., were taken after the stiffness tests, and special attention was paid to the hub and airscrew tightness, and it will be noted that the synchronous speed has been raised into the neighbourhood of 1600 r.p.m. The corresponding calculated stiffness from D to A is 174,000 lb. ft./rad. and a line representing this stiffness has been drawn through the centre of the hysteresis loop in Fig. (9b); this shows good agreement with the average stiffness for large torque fluctuations. It should be mentioned that the stiffness tests were made with the shaft in the engine. In other tests with a hub fitted to tapered splines, there was no hysteresis loop, but the deflection line was not quite straight near the origin. The importance of having well fitting splines and a satisfactory connection between airscrew and hub, is manifest. With short tapered splines the driving point cannot change so much, but the present method of bolting on the airscrew with large clearances round the bolts, leaves something to be desired from the engine standpoint, although the difficulty, from the airscrew standpoint, of making any change, is fully appreciated.

An abnormally springy connection here may lower a synchronous speed from a safe to a dangerous region, and as the human element comes into the matter of tightening the airscrew bolts, one fears that trouble may arise at any time from this cause where the synchronous speed is above the running speed range.

It is possible that differences in the stiffness or rigidity of the connection between airscrew and shaft may account for some anomalous differences of behaviour between apparently identical engine installations.

With a view to confirming that the correct interpretation had been placed upon the diagrams described above, the question of phase change was examined. It is a well-known fact which can easily be established mathematically that, in the notation adopted here, the angle of lag of the forced vibration behind the forcing vibration is $\tan^{-1} \Delta x/(1-x^2)$; so that the lag is 90°, or a quarter of a period, when $x=1$, irrespective of $\Delta$. With 4.5 vibrations per revolution, a vibration period corresponds to 80° of crank angle, and thus the phase lag on the diagram should correspond to 20° of crank angle at the speed of sychronism. In Fig. (9c) the lag calculated from the above formula is shown plotted on a base of engine speed, and experimental observations are shown for comparison plotted on the basis of agreement between theoretical and observed lag at 900 r.p.m., *i.e.*, in a region where error in $\Delta$ gives a relatively small difference in lag angle. The measurement of phase lag was made by reference to the sides of the

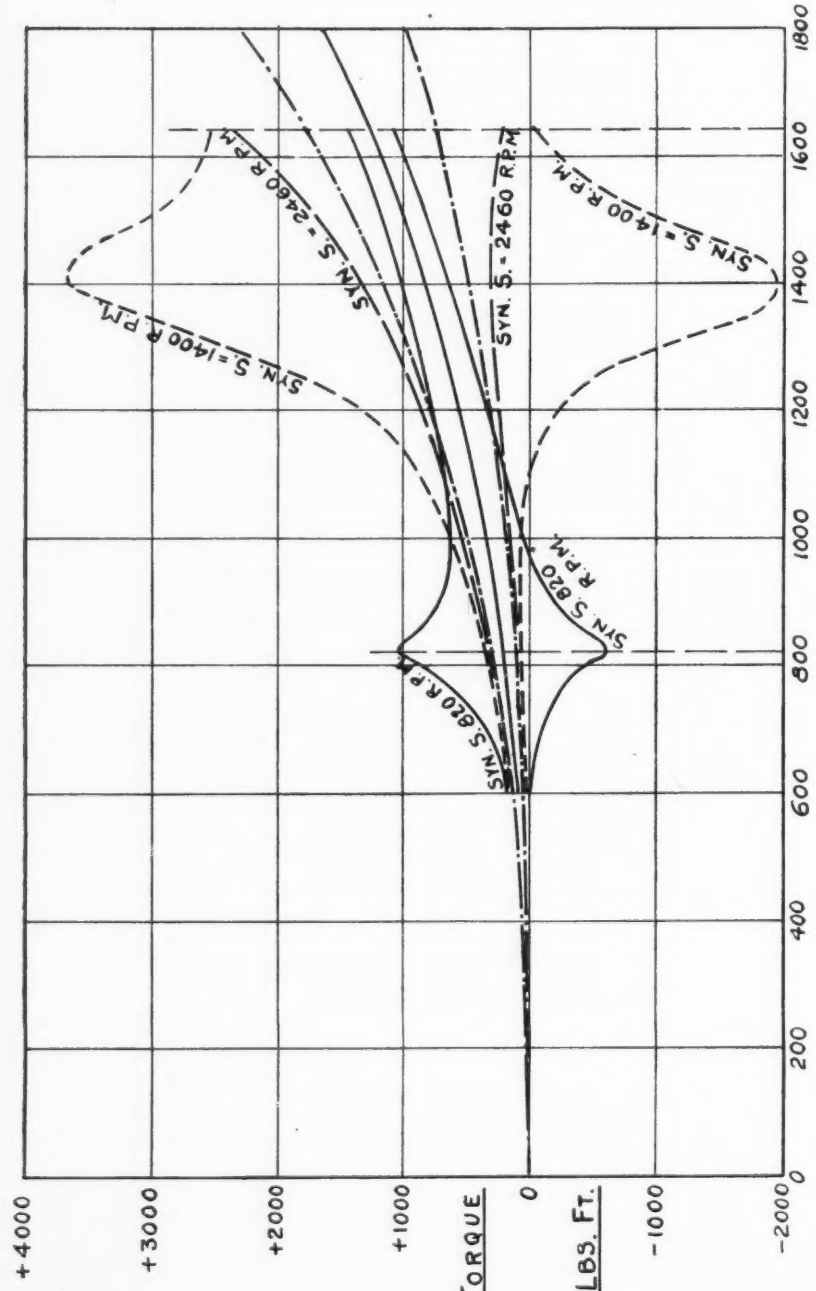
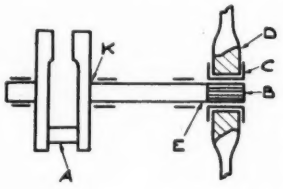

+4000
+3000
+2000
+1000
0
-1000
-2000
TORQUE
LBS. FT.
0
200
400
600
800
1000
1200
1400
1600
1800
SPEED IN R.P.M.
SYN. S. = 1400 R.P.M.
SYN. S. = 2460 R.P.M.
SYN. S. 2460 R.P.M.
SYN. S. 820 R.P.M.
SYN. S. 820 R.P.M.
SYN. S. = 1400 R.P.M.

Fig. 8.

cinematograph film, which involves the assumption that maximum gas-torque occurs at the same crank positions for all throttle openings and speeds, and takes no account of ignition setting, but this was not changed during the run: thus only rough agreement is to be expected. The results indicate that the synchronous speed is 1500 r.p.m., at which speed the phase lag is one quarter period. Other diagrams indicate a higher synchronous speed than 1400 and, as already mentioned, variations occur with differences of stiffness of the air-



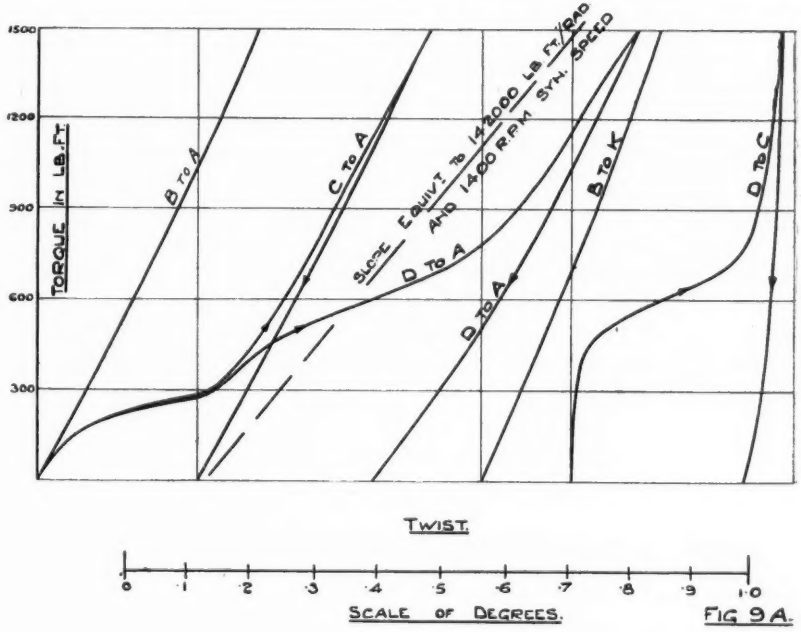




FIG 9A.

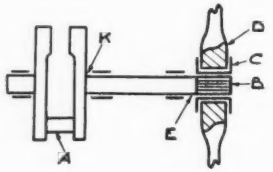
screw connection. The natural frequency of the instrument itself is very much higher than that of the crankshaft-airscrew system, and the instrument is too light to influence the engine vibration.

One could have wished to have obtained comparative diagrams at full throttle at different speeds before putting results before the Society.

Referring back to Fig. (7) it will be noticed that towards full speed the resonance factor is diminishing rapidly, and according to theory it should be less than unity beyond about 2000 r.p.m. By designing so that the synchronous speed is at, say, one half full speed (and running through it at low throttle),

the torque variation at full throttle should be reduced to about one-third of the gas-torque variation. This is analogous to running a shaft through its whirling speed.

The possibilities in this direction are strikingly indicated by Fig. (8). Here the parabolic hypothetical limits of gas-torque for the experimental engine are reproduced from Fig. (7), and superimposed are curves showing the theoretical



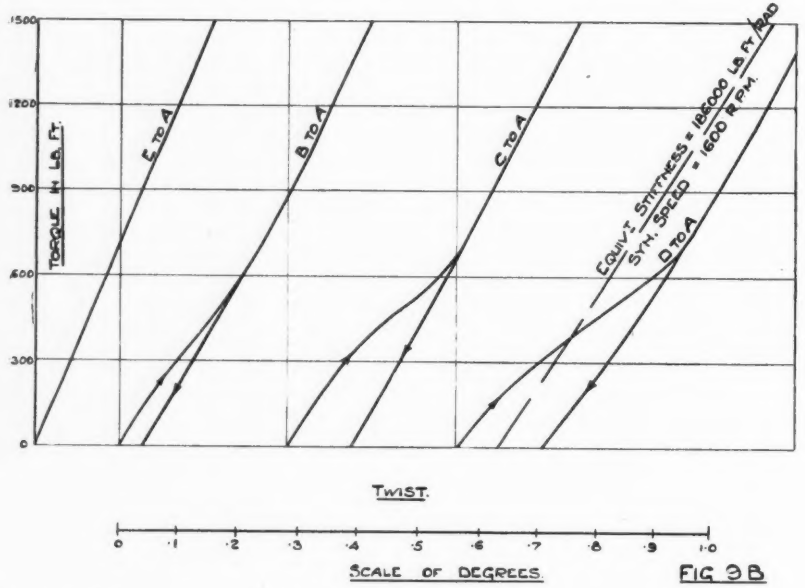




FIG 8B

torque limits in the shaft for two assumed cases, in both of which $\Delta$ is taken to be 0.12 :—

(1) Where the shaft stiffness is so reduced that the synchronous speed is lowered to 820 r.p.m., *i.e.*, to half full speed.

(2) Where the shaft stiffness is assumed to be so increased that the synchronous speed is raised to 2460 r.p.m., *i.e.*, to 50 per cent. above full speed.

It will be noted that the torque peak for Case (1) is about equal to the mean engine torque at full speed, and thus direct failure of the shaft should not occur at 820 r.p.m. in a shaft designed for indefinite running at or near full speed and power, and the running at 820 r.p.m. would be too brief for fatigue failure to be brought about by the torque reversal.

The large torque variation at full speed for Case (2) compares very unfavourably with the corresponding variation for Case (1). Not only is the comparison adverse from the point of view of crankshaft stresses, it is also adverse in respect of big-end loading, as is shown later.

*Experimental and Calculated Phase Changes.*

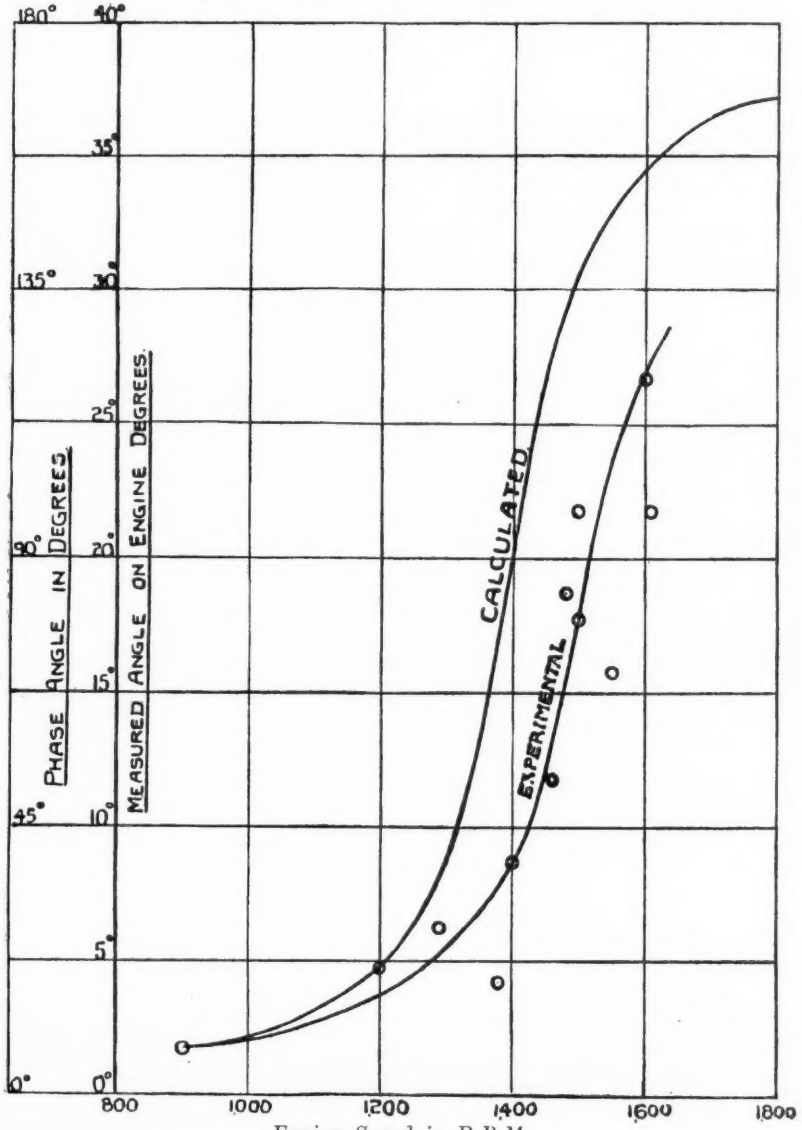



FIG. 9c.

### *Calculation of the Main Synchronous Speed*

With $n$ cylinders equally spaced working on the four-stroke cycle, the frequency per second of the main explosion impulse is $n \times$ r.p.m./120, and at the synchronous speed this is equal to the natural frequency, namely,

$$(1/2\pi)\sqrt{\{C_s g (1/I_c + 1/I_a)\}} \text{ per sec. as given on page 279.}$$

It may be objected that a crank with balance weights, connecting rods, wrist pins, gudgeon pins and pistons, and auxiliary drives, is too unlike a flywheel to be assumed equivalent to one, but a little consideration will show that the assumption is justified.

Imagine the airscrew to be fixed and a tangential force applied at the crankpin mid-length. The crankshaft, webs and crankpin will yield elastically, causing corresponding moments of connecting rods and pistons, and strain energy will be stored in the system. On removing the force the various parts will swing back into unstrained relationship and at the instant this condition is attained the total kinetic energy of the various masses must be equal to the energy previously stored—less the loss by damping, which has little effect on frequency. On this basis the above equation for frequency may be obtained if the kinetic energy is completely represented by rotation about the crankshaft axis: in as far as there are motions about other axes, however, the velocities about the crank axis will be less and the frequency lower. Now the principal motions corresponding to the kinetic energy are :—

(1) Angular velocity of the parts of the crank itself, comprising pin, webs, and balance weights.
(2) The motion of the big-end assembly.
(3) The motions of the several reciprocating masses.
(4) The motions of auxiliaries driven from the crank, by magnetos and valve gear.

Each of these will be commented upon in turn.

1 *Motions of Crank*

If the crank were quite rigid it would obviously be equivalent to a flywheel, but actually the motion is complex. One crank web may swing back through a larger angle and with a higher velocity than the other; also, the balance weights may " waggle " about their axes of symmetry owing to the twisting of the webs. It appears that the first point may be taken into account sufficiently well for practical purposes by measuring the shaft stiffness up to the crankpin centre, the angular movement of which may be taken as the average of the angular movements of the crank webs.

To determine whether the second point corresponds to an appreciable increase in the effective inertia of the system, corresponding distortion measurements were made on a radial engine crankshaft, and from these it has been concluded that the effect is quite negligible.

2 *Motion of Big End Assembly*

(*a*) If the big-end kept parallel to itself during rotation of the crank, it would be equivalent to a concentrated mass at the crankpin centre, *i.e.*, its own polar moment of inertia would not affect the vibration. With a master rod and articulated rod system there are obliquity movements making the effective inertia greater near dead centre positions. This has been ignored hitherto and as no appreciable error appears to have been involved thereby, it is believed that the effect is small. The average inertia increase for all crank positions could be calculated for any given case.

(*b*) There is clearance in the big-end bearing and in all bearings of the crank-connecting-rod piston system, but the movements can be shown to be too small to affect the vibration appreciably. The effect of big-end clearance is investigated later.

3 *Reciprocating Masses*

Some reciprocating masses will be moving faster than the others according to their positions relative to the crank. It is not difficult to prove that with a slipper type big-end, the total kinetic energy of all the reciprocating masses is that represented to a first approximation by one half of their total mass concentrated at the crankpin centre, so that the frequency of vibration would be the same if the reciprocating parts were replaced by such a mass. With a master rod system the same appears to apply with sufficient accuracy for practical purposes, but this matter requires examining more fully.

4 *Driven Auxiliaries*

Rough calculations serve to show whether the inertia of these is sufficient to matter. Where the drive is non-rigid the effects may be complex and not easy to calculate. The special case of a driven damper, or a driven supercharger is referred to later.

---

From the foregoing considerations the following practical rule has been adopted for estimating the value of $I_e$.

Calculate or determine experimentally the moment of inertia about the crankshaft axis of the crank itself, comprising webs, pin, and balance masses.

Take the big-end mass plus one half the reciprocating mass and multiply it by the square of the throw.

Add these two quantities to obtain $I_e$.

It remains to find the value of $C_s$. When the engine exists it is best to determine this by making a static twisting test as between the airscrew and the crankpin centre in the engine crankcase. Otherwise the stiffness may be found by a process of calculation and judicious guess work from the results of stiffness tests available on similar shafts. The calculation of shaft stiffness is by no means an exact science and there is scope for some useful work in the direction of establishing formula that can be used with confidence in this matter. One uncertain feature is the rigidity of the crankcase to be assumed, and its relation to shaft stiffness.

*Example*

$I_a = 230$ lb. ft.$^2$. $I_e = 11$ lb. ft.$^2$.

$C_s = 186{,}000$ lb. ft./rad. from airscrew to crankpin centre.

Natural frequency $= (1/2\pi)\sqrt{\{\, 32.2 \times 186{,}000\,(1/11 + 1/230)\,\}}$

$= 120$ per second.

With nine cylinders equally spaced working on the four-stroke cycle the speed for equal frequency of explosion impulses is $120 \times 60/4.5 = 1600$ r.p.m.

The quantity $I_e$ is made up as follows :—

| | lb. in.$^2$ |
|---|---|
| M. of I of crank and balance masses (determined by swinging on knife edges) ... ... ... ... ... ... ... ... | 1080 |
| ,, Big-end with wrist pins and portion of connecting rods | 240 |
| ,, Half of total reciprocating mass (Pistons, G. Pins and remainder of C. rods) ... ... ... ... ... | 260 |
| | 500 |
| Total | 1580 |

### *Effects of Driving a Damper or a Supercharger from the Tail End of the Crankshaft*

This matter is too involved to consider here in any detail, but some general results of an investigation made in relation to single-throw engines may be given.*

The investigation in question applies also to multi-crank engines in which the crank portion is very stiff relatively to the connection between it and the airscrew or main flywheel. Fig. (10) depicts the system considered.

The problem is to determine the nature of the forced vibration at any speed for different relations between the quantities involved, and the power absorbed in damping. For single throw engines stiffnesses $C$ and $C'$ are reckoned to the crankpin centre.

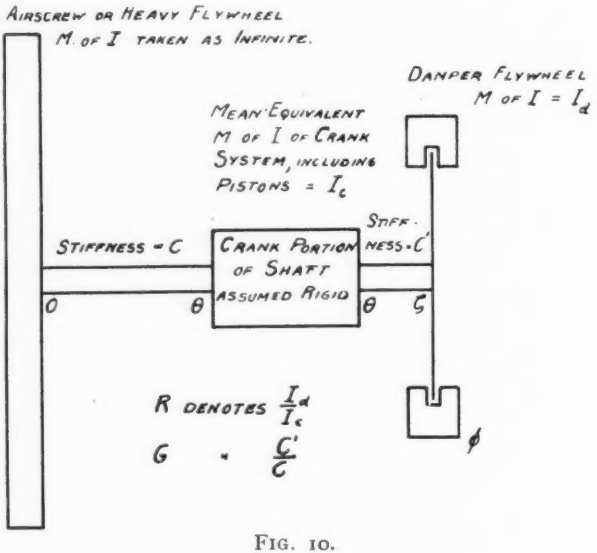



FIG. 10.

The resonance factor for the crankshaft is modified by the addition of a damper from the simple expression on page 280 to the much more formidable one

$$|\theta|/E = \sqrt{\{1+(1/Rx-x/G)^2D^2\}/\sqrt{\{(1-x^2)^2+\Delta^2x^2+2D\Delta x^2+D^2[x^2-2(1/R-x^2/G)(1-x^2)+(1/R-x^2/G)^2\{(1/x-x)^2+\Delta^2\}]\}}}$$

where $R$ is the ratio of the damper flywheel inertia to $I_c$.

$G$ is the ratio of the damper-drive stiffness to $C$.

$D$ is a non-dimensional coefficient of damping for the damper, which is assumed to be of the viscous friction type, but the results may be applied to some extent to dampers of the solid friction type.

From this expression simpler ones may be derived for various special cases, certain of which will be considered briefly.

* For fuller particulars see R. & M. 1053, by the Author, in which the same notation is used. The functioning of vibration dampers is examined in articles by Prof. Dr. Eng. L. Gumbel in *Zeitschrift des Vereines Deutcher Ingenieure*, March 18th and 25th, 1922.

### *Case 1 Rigidly Driven Damper*

1. (*a*) *Resonance Factor.*—If the damper drive be very stiff and damping in the engine be neglected, we may put $G=\infty$ and $\Delta=0$: and if, for example, the damper flywheel be equal in inertia moment to the crank mass, we may put $R=1$. The resonance factor then becomes

$$|\theta|/E = \sqrt{(1+D^2/x^2)} / \sqrt{\{(1-x^2)^2+D^2(2x-1/x)^2\}}$$

This is shown plotted in Fig. (11) for a series of values of $D$. The significance of $D$ being zero is that the damper friction is zero; and $D=\infty$ means that the damper flywheel is seized or locked solid, so that its inertia is added to the crank mass.

When $D=0$ the damper has of course no influence, and the resonance curve is that already given in Fig. (2) for $\Delta=0$.

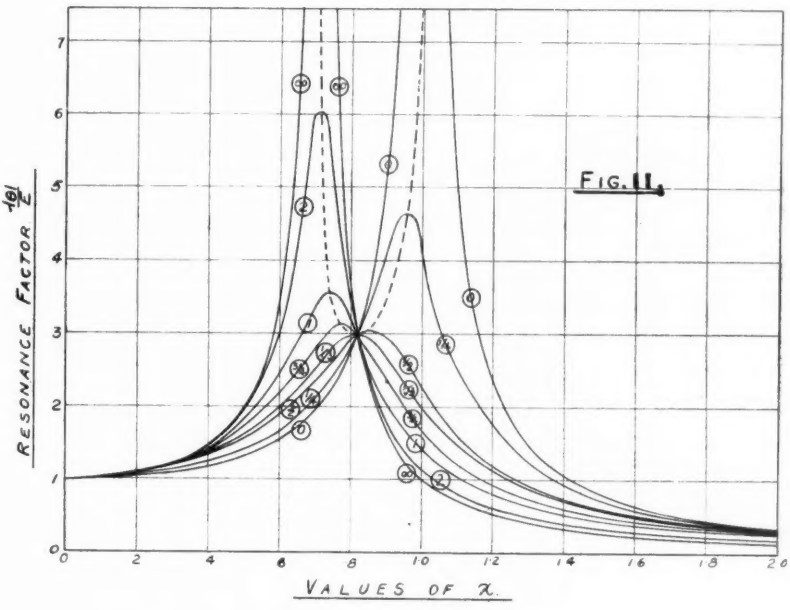



FIG. 11.

CASE WHERE G = ∞
R = 1
Δ = 0
D = VALUES SHOWN
LOCUS OF MAXIMA

As $D$ is increased, the peak occurs at a lower speed and its height diminishes at first to a minimum value, namely, 3 when $D=1/\sqrt{3}$, and then increases to infinity when $D$ is infinite. As $I_c$ has become doubled by making $D$ infinite, the value of $x$ for this infinite peak is $1/\sqrt{2}$; if $R$ were greater the infinite peaks would be wider apart.

Similar curves are obtained if damping within the engine is taken into account, only the extreme peaks are not infinite but of height equal to $1/\Delta$.

From the foregoing it will be seen that the effect of adding a damper is to diminish the resonance factor at the original synchronous speed progressively with increased damper friction, but this is brought about by moving the resonance peak to a progressively lower speed.

1 (*b*) *Horse-power Dissipated by the Damper.*—It can be shown that if as in (*a*) above $G=\infty$ and $\Delta=0$ the ratio of the horse-power dissipated in the damper, to the engine horse-power, is:—

$$nEDBx/4T\left[(1-x^2)^2+D^2\left\{(1+1/R)x-1/Rx\right\}^2\right]$$

where $n$ is the number of (four-stroke) cylinders and $T$ lb. ft. is the mean engine torque.

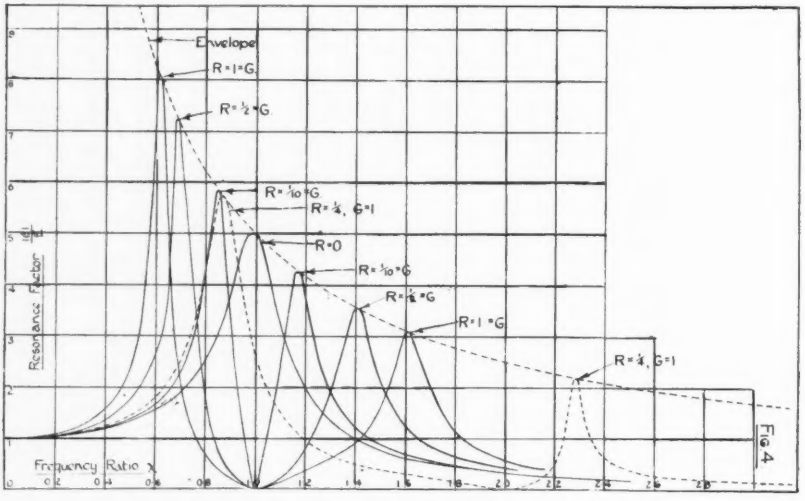
The large values of the ratio when $x=1$ and $D$ is very small arise from the fact that damping within the engine is neglected in the formula—the evolution of a formula to take into account the value of $\Delta$ should present no difficulty. When determining the power dissipation for any conditions, the corresponding resonance factor needs to be determined also—for the rate of energy dissipation may correspond to an amplitude of shaft movement impossible to realise on account of shaft failure intervening.

## *Case 2 Elastically Driven Flywheel or Supercharger*

2 (*a*) *Resonance Factor.*—For such $D$ is infinite and the resonance factor is:

$$|\theta|/E=1/\sqrt{\left[\left\{x^2\left(1/\left[1/R-x^2/G\right]+1\right)-1\right\}^2+\Delta^2x^2\right]}$$

In Fig. 12 this is shown plotted for various values of $R$ and $G$ taking $\Delta=0.2$.




*Graph Relating to Main Driving Shaft.*

FIG. 12.

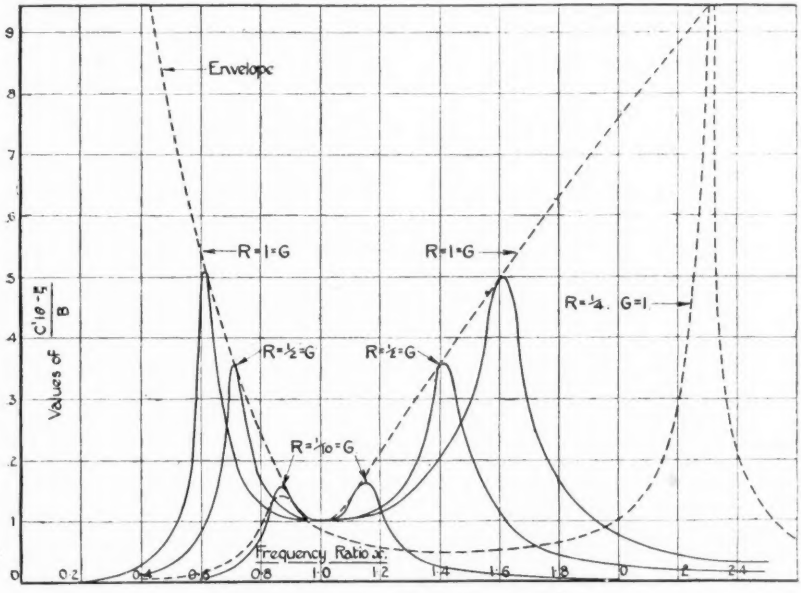
When $R=0$ we get the single peaked resonance curve for the engine alone as for $\Delta=0.2$ in Fig. 2.

When $R=G$ the peak is divided into two peaks at reciprocal values of $x$, which are further apart, the greater the common value of $R$ and $G$, and which all touch an hyperbola as envelope.

The significance of $R$ being equal to $G$ is that the natural frequency of the added system about the crank mass held fixed, is equal to the natural frequency of the engine alone. Theoretically, provided this relation holds, the resonance factor dips down to zero value when $x=1$, however small the added flywheel—but for a very small flywheel the amplitude of swing would be very great, sufficient in fact to produce torque $B$ in a very flexible shaft.

When $R=G=\frac{1}{2}$ (say) there is a fairly wide speed region about $x=1$ for which the resonance factor is less than unity.

If $G$ is not equal to $R$, the peaks are no longer at reciprocal values of $x$, and the value of $x$ for zero resonance factor is no longer unity but $\sqrt{(G/R)}$. Thus, if the drive is relatively stiff and $R=\frac{1}{4}$, $G=1$, $|\theta|/E$ is zero when $x=2$, and one peak is near the $R=0$ peak with $x$ less than unity, while for the other peak $x$ is about 2.3.




*Graph Relating to Added-Flywheel Drive.*

FIG. 13.

On the other hand, with a relatively elastic drive, so that $G$ is less than $R$, one peak occurs at a low value of $x$ and the other is near the $R=0$ peak with $x$ greater than unity.

2 (*b*) *Torque in Drive.*—A somewhat similar set of curves applies to the torque amplitude in the added flywheel or supercharger drive and these are just as much to be considered.

Fig. 13 shows a set of curves corresponding to those of Fig. 12 for $\Delta=0.2$. The ordinates denote the ratio of the torque amplitude in the drive to $B$, the amplitude of gas torque variation. In the region $x=1$, the amplitude of torque variation in the drive approximates to that of the gas torque, and there are peaks on either side reaching to an envelope curve as shown.

Some information respecting the functioning of a solid friction damper may be obtained from these curves, from the consideration that slipping will not occur for values of $x$ corresponding to a torque amplitude less in value than the torque required to overcome the damping friction as set.

The effects of adopting spring drives of different stiffnesses to a supercharger of known equivalent inertia moment may be estimated from the curves of Figs. 12 and 13, or similar ones. If the supercharger have a gear drive, the equivalent inertia moment of the rotor beyond the gears is the actual inertia moment multiplied by the square of the gear ratio.

### *Case 3 Elastically Driven Damper*

Here neither $G$ nor $D$ is infinite and thus neither quantity can be eliminated from the equations, the complexity of which renders interpretation difficult. The matter has been investigated within limits, however, by confining attention chiefly to cases for which $G=R$, and it has been inferred that by adopting a suitable stiffness for the damper drive a much lighter damper may be made effective than when the damper connection is rigid, whether the conditions necessary to gain this advantage could be maintained in practical applications is open to question.

## *Some Effects of Big-End Clearances in Single-Throw Radial Engines*

### *(a) Analysis of Motion*

The resultant inertia force on the big-end tends to keep it radially outwards against the crankpin. This force is opposed periodically by gas forces, which also introduce a crank effort component, the variations of which will now be considered.

Let $\theta$ define crankpin positions, and $\phi$ big-end positions, about the crankshaft axis relative to co-ordinates rotating uniformly at mean crank speed. Then $\theta-\phi$ is a very small quantity which can vary only within the limits imposed by the big-end clearance and oil film thickness. It will be assumed that the oil film, in being squeezed in and out, gives rise to a viscous resistance, proportional at each instant to $\dot{\theta}-\dot{\phi}$, and defined by a non-dimensional coefficient $D$ as in the case of the damper. It will be assumed also that the principal damping apart from $D$ occurs between the big-end system and the engine cylinders, and that it is proportional to $\dot{\phi}$, being represented by a non-dimensional coefficient $\nabla$

Harmonic engine impulses of torque amplitude $B$ are assumed as before and symbols previously used are introduced with the same meanings.

By putting down the differential equations of motion and solving them it has been found that the resonance factor for the crankshaft is:—

$$|\theta|/E = I/\sqrt{\{[1-x^2+(\nabla/D)(1-\rho x^2)]^2+[\nabla x+(1-\rho)x(1-\rho x^2)/D]^2\}}$$

where $\rho I_c$ is the inertia of the crank webs, pin and balance weights without the big-end, etc. (In the case taken on page 295, $\rho=1080/1580=0.68$.) When there is no relative lateral movement of crankpin and big-end, $D$ is infinite and the above equation takes the form

$$|\theta|/E = I/\sqrt{\{(1-x^2)^2+\nabla^2 x^2\}}$$

irrespective of the value of $\rho$.

When $D$ has any arbitrary value, and $x=1$, the resonance factor is

$$|\theta|/E = I/\sqrt{[\nabla^2(1-\rho)^2/D^2+\{\nabla+(1-\rho)^2/D\}^2]}$$

which becomes $I/\nabla$ when $D$ is very large, and (as will be seen later) does not depart much from $I/\nabla$ for values of $D$ that can obtain.

Again, when $x=\sqrt{(1/\rho)}$, the resonance factor has a value independent of the value of $D$, namely:

$$|\theta|/E = 1/\{(1/\rho-1)^2+\nabla^2/\rho\}^{\frac{1}{2}}$$

Thus if $\rho = .68$, the resonance factor when $x = 1.21$ is

$$|\theta|/E = 1/\{0.22 + 1.47\,\nabla^2\}^{\frac{1}{2}}$$

whatever the value of $D$, which is 2.13 when $\nabla = 0$ and 1.9 when $\nabla$ is as large as 0.2, so that the resonance factor curves on $x$ as base for different values of $D$ all pass very nearly through the point $x = 1.21$, $|\theta|/E = 2$, for values of $\nabla$ that matter.

If $|\theta - \phi|$ denote the amplitude of $\theta - \phi$, it can be shown that

$$|\theta - \phi| = (1 - \rho x^2)/x \,.\, |\theta|/E \,.\, B/C.$$

This is zero when $x = \sqrt{(1/\rho)}$ and the crank effort is constant at its mean value at the corresponding engine speed; the mechanical interpretation of this is that the gas torque variation is just that necessary to vibrate the big-end and piston system with amplitude $|\theta|$ at the frequency corresponding to $X = \sqrt{(1/\rho)}$, while the crank swings undamped and no longer "forced." It may be expected that, were damping in the crankshaft taken into account, the equations obtained would give some crank effort variation at all values of $x$, but that the variation would be small in the region of $x = \sqrt{(1/\rho)}$.

Now $|\theta - \phi|$ has a peak value very near $x = 1$, when

$$|\theta - \phi| = (1 - \rho)/D \,.\, |\theta|/E \,.\, B/C.$$

If the synchronous speed of the engine considered is at 1,400 r.pm., $C$ is 142,000 lbs. ft., $B$ is about 380 lb. ft. and $|\theta|/E = 8.25$ when $x = 1$. Putting $\rho = 0.68$ we get maximum

$$|\theta - \phi| = 0.32 \times 8.25 \times 380/142{,}000\, D = 1/140\, D \text{ radian.}$$

Whence it appears that $D$ is about 10, since this value makes $|\theta - \phi|$ equal to 1/1400, corresponding to 1.4/1000 inch big-end clearance on the pin diameter per inch of crank throw, which is of the right order of magnitude. Thus $D$ can hardly be less than 10, and larger values affect results little so that $D$ may be taken as 10 without great error.

If $\nabla = 1/9$, the crankshaft resonance factor for $x = 1$ is

$$|\theta|/E = 1/\sqrt{\{(.32/90)^2 + (1/9 + .32^2/10)^2\}} = 8.25$$

which agrees with experimental determination and $\nabla$ will consequently be taken as 1/9.

### (*b*) *Power Absorbed in Damping*

If h.p.$_D$ denote the horse-power absorbed in the big-end bearing on account of coefficient $D$, it can be shown that with $n$ cylinders and $T$ lb. ft. mean torque :—

$$\text{h.p.}_D\text{/engine horse-power} = n/4 \,.\, B/T \,.\, (E/Dx)\,(1 - \rho x^2)^2 \,.\, (|\theta|)^2/E$$
$$= 1/58\, D \text{ when } x = 1 \text{ for the case taken.}$$

This is zero when $D$ is infinite and 0.17 per cent. when $D = 10$. The engine horse-power is about 250 at 1400 r.p.m. and thus, according to this working, 0.42 horse-power is absorbed in big-end crank effort damping at the synchronous speed.

Again, if h.p.$_\nabla$ denote the horse-power absorbed in the connecting rod system damping, on account of the coefficient $\nabla$, the following formula may be established :—

$$\text{h.p.}_\nabla\text{/engine horse-power} = n/4 \,.\, B/T \,.\, \nabla\, Ex\,[1 + (1 - \rho x^2)^2/(Dx)^2] \,.\, (|\theta|/E)^2$$
$$= (\nabla/6)\,\{1 + (.32/D)^2\} \text{ when } x = 1 \text{ for the case taken,}$$
$$= (1 + .0102)/54 = 1.9 \text{ per cent. when } \nabla = 1/9 \text{ and } D = 10.$$

This gives 4.7 horse-power dissipation corresponding to $\nabla$

The total power dissipated in damping is thus 5.1 h.p. or 2.1 per cent. of the engine horse-power, which is equivalent to 217 B.T.U.'s per minute—suffi-

cient to raise the temperature of the crank and connecting rod system at the rate of about 20°F. per minute if none of it were conducted away. On page 285 the dissipation between shaft and airscrew is estimated at 2 horse-power or two-fifths of the whole, and there is still 2.7 horse-power dissipation to be accounted for by the coefficient $\nabla$

From the large hysteresis loop necessary to represent 2 horse-power, it is difficult to attribute this 2.7 h.p. to hysteresis, and it may be lost chiefly by oil film dissipation at the various bearings; on the other hand, vibration of the shaft held fixed at the airscrew end and vibrated by suddenly disconnecting a mass hung from the crankpin (the crank being horizontal) showed a large logarithmic decrement, which indicates that hysteresis can account for much—but the rigidity of the hub connection is a matter in doubt for these vibration tests.

The quantities worked out above, by way of example, must be regarded as approximate only; further experiments are needed to get more exact figures.

If $D$ were infinite and $\nabla$ were 1/8.25, we should have the same $|\theta|/E$ when $x=1$ and, in fact, the virtual big-end clearance would be zero. This being the case the damping $\nabla$ need not be regarded as occurring in the connecting rods, etc., only, but may occur partly or wholly in the crankshaft, without affecting the mathematics, so that $\nabla$ is synonymous with the symbol $\Delta$ used previously. Thus we get:

$$\text{h.p.}\Delta/\text{engine horse-power}=\Delta/6=0.020 \text{ or } 2 \text{ per cent.},$$

which is practically the same as before with $D$ equal to 10.

### (c) *Crank Effort Variation*

The amplitude of crank effort variation may be shown to be

$$F\,|\dot{\theta}-\dot{\phi}| = B\,(1-\rho x^2)\,(|\theta|/E)$$

which is a maximum when $x$ is slightly less than unity, but the maximum value will be obtained sufficiently accurately by putting $x=1$, when $|\theta|/E=8.25$ for the case taken.

$$\text{Then } F\,|\theta-\dot{\phi}| = .32\times 8.25\,B = 2.7\,B.$$

The interpretation of this is that at the critical speed the variation of crank effort on the crankpin is 2.7 times that corresponding to the variation of gas torque. As the gas torque variation is about 40 per cent. of the average, this means that at 1,400 r.p.m. the tangential force on the crankpin varies at explosion frequency between about zero and twice the average value. Whether this represents abnormally severe conditions for the bearing is a matter to be worked out by combining tangential and radial forces to get the resultant in a number of crank positions.

This matter is not so straightforward as it may appear, however, for phase change must be taken into account in combining the components.

Probably conditions of resonance which are severe enough to be of importance from the standpoint of big-end loading are not tolerable from the standpoint of crankshaft stresses.

The above equation also shows that the tangential load on the crankpin is constant when $x=\sqrt{(1/\rho)}$, as mentioned previously.

As $x$ increases from $\sqrt{(1/\rho)}$ to 2 (*i.e.*, from 1.21 to 2 in the case taken), $F\,|\dot{\theta}-\dot{\phi}|$ increases from zero to $.56\,B$. Thus running in the region above the synchronous speed does not reduce crankshaft stresses at the expense of increased big-end loadings but, on the contrary, the crank effort variation is affected favourably—a conclusion which helps to justify this investigation from the practical standpoint.

With a roller-bearing bearing big-end, yielding of the crank may upset bearing alignment and resonance will accentuate this.

### *Torsional Vibration in Multi-Crank Engines*

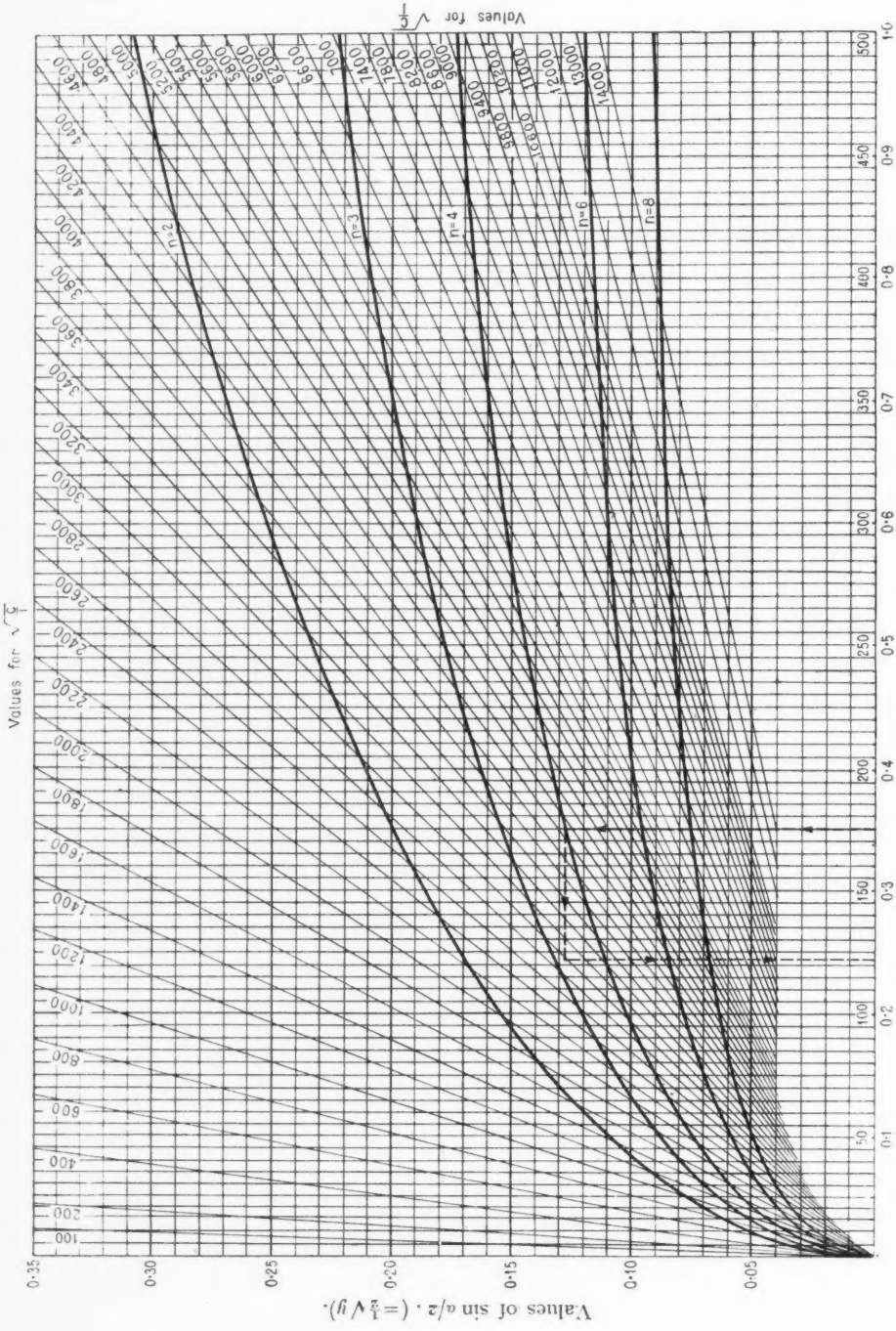
The mathematical investigations made in relation to the driving of a flywheel or supercharger from the tail end of the crankshaft of a single throw radial engine can be applied with little modification to the case of the two-throw radial engines. It appears that in such engines the resonance effects may be kept small by making the stiffness between the crankpin centres greater than that between the airscrew and the first crankpin centres; thus it is important that the intermediate crank web be made stiff, and probably the provision of a central bearing would affect the shaft loadings adversely. It is not proposed to go into this matter here, however, but to consider briefly the general problem of resonance in multi-throw engines. Where the connection between the airscrew or heavy flywheel is very flexible as compared with the connection between adjacent crankpin centres (as with a ball bearing crankshaft engine with a long driving shaft, and possibly with reduction gearing), there is only one mode of torsional vibration that matters, namely, that in which there is a node near the airscrew hub and the crank masses swing together. The case is then similar to that of the single-throw radial engine, and the natural frequency may be obtained to a first approximation in the same manner, putting the total crank inertia together including that represented by one half of the reciprocating masses acting at crank radius. The methods mentioned later may be applied, however, to get a more exact value. In such a case the determination of natural frequency does not involve the possibility of grave error, for the stiffness of the shafting that has most influence on the frequency may be computed with fair accuracy from the drawings, and moreover the spring in the connection between shaft and airscrew may be relatively unimportant.

With a plain bearing shaft having a short stiff length of shafting between the hub and the first crank web the case is different, for here the chief unknown is the stiffness of the crankshaft itself, which does not lend itself readily to calculation.

The roughest approximation for finding the natural frequency in such a case is to take the shaft with its big-end masses to be equivalent to a system comprising a light shaft of the same over-all stiffness carrying at the end remote from the airscrew, a flywheel of one third of the total inertia. Although this may be found to give good results in some cases, the method cannot be considered satisfactory. A better approximation is to take each crank to be equivalent to a flywheel of inertia value as calculated for the single-throw radial engine and to determine the stiffness between adjacent crankpin centres and between the airscrew and first crankpin centre. Then from equations for flywheel systems obtained by the method of Chree, Millington and Sankey,* the natural torsional frequencies of the system may be deduced. In actual cases all the throws are equal and the average stiffness between crankpin centres may be taken to apply all along the shaft. This leads to simpler expressions for determining the natural frequencies and these have been expressed by Captain J. Morris in a very concise form.†

Fig. 14 is a chart reproduced from an article by Captain A. Swan and the Author in *The Automobile Engineer*, March, 1926, by kind permission of the Editor. It gives the lowest natural frequency of engines having two, three, four, six and eight cranks in line, when fitted with an airscrew or flywheel of high inertia moment.

* See *Proceedings Inst. Mech. Engineers*, 1904; also Appendix to this lecture.
† *Journal Royal Aeronautical Society*, April, 1923, and October, 1923.


Values for $\sqrt{\frac{\rho}{q}}$
Values of sin $a/2$ . $(=\frac{1}{2}\sqrt{y})$.
Values of $q$.
n=2
n=3
n=4
n=6
n=8
100
200
400
600
800
1000
1200
1400
1600
1800
2000
2200
2400
2600
2800
3000
3200
3400
3600
3800
4000
4200
4400
4600
5000
5200
5400
5600
5800
6000
6200
6600
7000
7400
7800
8200
8600
9000
9400
9800
10200
10600
11000
11400
12000
14000
50
100
150
200
250
300
350
400
450
500
0·1
0·2
0·3
0·4
0·5
0·6
0·7
0·8
0·9
1·0
0·05
0·10
0·15
0·20
0·25
0·30
0·35

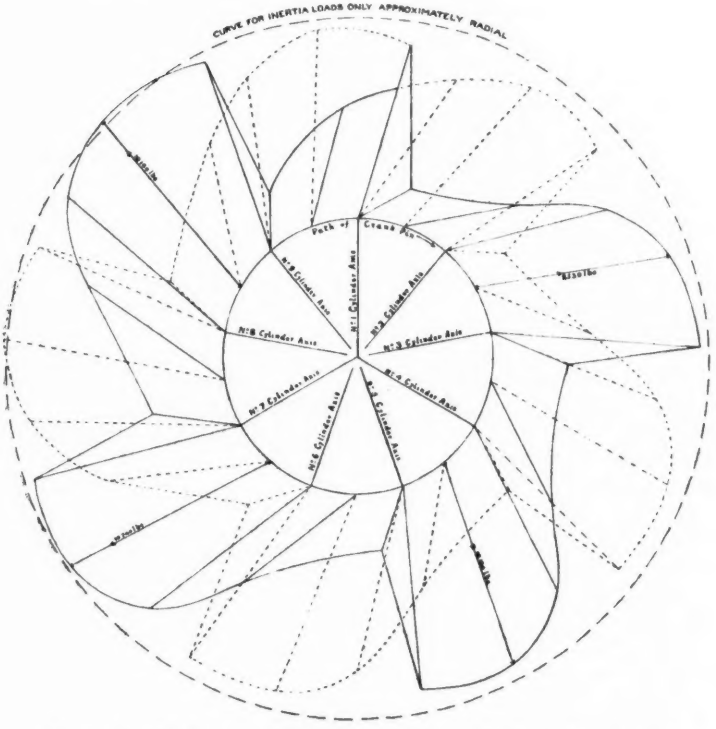
The quantities involved are:—

*C* The stiffness between adjacent crankpin centres, in poundal feet per radian.

*I* The moment of inertia of the crank masses belonging to one throw—in lb. ft.

*n* The number of throws.

*q* The ratio: Torsional stiffness between airscrew and crankpin centre of the first throw/Torsional stiffness between adjacent crankpin centres.




*Dragonfly Engine.* 9 *Cylinders,* $5\frac{1}{2}''$ *bore stroke,* 1,650 *r.p.m.*
*Inertia and Total Loads on Big-End Bearing.*
*All Forces Measured from Crank Pin Circle.*

FIG. 15.

To find the frequency per second, start with the abscissa value of $q$ and follow the ordinate to the appropriate curve for $n$, thence along horizontally to the line for $\sqrt{C/I}$ and down vertically to the base for the frequency value.

For the fictitious example indicated on the chart, $C = 137.5 \times 10^6$ poundal feet and $I = 16.35$ lbs. ft.$^2$, so that $\sqrt{C/I} = 3000$; $q$ is 0.35; $n$ is 4; and the frequency works out at 122 per second. For a four-stroke engine of eight cylinders the corresponding synchronous speed would be:—

$$(122 \times 60)/4 = 1{,}830 \text{ r.p.m.}$$

The natural frequency of the corresponding six-throw engine would be 148 per second, and if there were twelve four-stroke cylinders the synchronous speed would be $148 \times 60/6 = 1{,}480$ r.p.m. If to this balance masses were fitted such as to increase 1 to 20 lb. ft.$^2$ the new value of $\sqrt{C/I}$ would be 2,720, whence the modified frequency would be 135 per second and the synchronous speed 1,350 r.p.m.

These examples suffice to show that the effects on the main synchronous speed of proposed alterations in design may be estimated readily from the chart. The lowest frequency corresponds to vibration about a single node at or near the airscrew. Vibrations with two or more nodes tend to occur at speeds which are too high to be of importance, but when the main synchronous speed is low the possibility of synchronism with other than the single node vibration should not be overlooked and recourse should be had to the appropriate formula. The flywheel system is equivalent to a system of electrical circuits* and the analogy provides an alternative method of obtaining the several natural frequencies which are equal in number to the number of throws. In complex cases it may be worth while making up the corresponding electrical circuit and obtaining the results by electrical experiments.

The equations given in the Appendix cover most orthodox engine arrangements, however, and the chief problem is that of obtaining reliable values for stiffnesses.

The tedium of determining the roots of these equations may be obviated by plotting appropriate curves from which the root values may be read off in any particular case. Thus, taking equation (13) (page 308) relating to six-throw engines, we may write it in the form:—

$$q = (y^6 - 10\,y^5 + 36\,y^4 - 56\,y^3 + 35\,y^2 - 6\,y)/(y^5 - 9\,y^4 + 28\,y^3 - 35\,y^2 + 15\,y - 1)$$

The expression $q$ could be computed for a series of values of $y$ and the results plotted against $\sqrt{y}$, bearing in mind that the natural frequencies are

$$(1/2\pi)\sqrt{(C/I)}\sqrt{y}.$$

In this way six curves would be obtained giving six values of $\sqrt{y}$ for each value of $q$.

Now in Captain Morris's notation, the frequencies are:—

$$1/2\pi\sqrt{(C/I)}\,(2\sin\alpha/2) \text{ where } 1/\{1 + \cot(\alpha/2)\cot n\alpha\}\,q.$$

Thus the quantity $\sin\alpha/2$ plotted in Fig. 14 is identical with $\frac{1}{2}\sqrt{y}$ and the trigonometrical expression for $q$ is identical with the algebraic one corresponding to the same value of $n$, the number of throws. The trigonometrical form provides a means of reducing the computing work involved in preparing curves for subsequent use.

The curves of Fig. 14 are those for the lowest frequencies, *i.e.*, for single node vibration. Similar graphs could be prepared for two-node vibration, three-node vibration, and so on.

The plotting of radial lines for various values of $\sqrt{C/I}$ provides a convenient means of multiplying

$$\sin\alpha/2 \text{ (or } \tfrac{1}{2}\sqrt{y}) \text{ by } (1/\pi)\sqrt{(C/I)}$$

to get the corresponding frequency values.

From the results of stiffness tests made on shafts of various proportions it is possible to make a fair estimate, but in important cases the stiffness should be checked by tests on the shaft itself, if there appears any likelihood of the main synchronous speed coming in an unsatisfactory speed region. Harmonics of the engine torque will synchronise with the natural frequencies at $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, etc., of

* See R. & M. 1045, by E. B. Moullin, M.A.

the speeds for synchronism with the fundamental impulses. The effects of these are of secondary importance however.

The Author is indebted to the Director of Research, Air Ministry, for permission to give this lecture. It should be understood that opinions expressed therein and in reply to the discussion are personal.

In conclusion, I desire to express my appreciation of the assistance received from my colleagues at the Royal Aircraft Establishment in making the investigations described, and in particular that received from Captain Andrew Swan, who has been closely associated with me in this work.

## APPENDIX

### *Frequency Equations for Various Airscrew-Crankshaft Systems*

#### *Notation Relating to this Appendix Only*

$I$ denotes, with suffixes 1, 2, 3, etc., the moments of inertia of the rotating masses.
$l$ denotes the lengths of the shafts between the rotating masses.
$d$ denotes the outside diameter of the shaft.
$d_0$ denotes the internal diameter of the shaft where hollow.
$n$ denotes the modulus of rigidity of the material of the shaft.
$J$ denotes the polar moment of inertia of the shaft section.
$C$ denotes the stiffness ($nJ/l$) of the shaft.
$1/t = k/2\pi$ denotes the frequency of natural vibration of the system $ay = k^2$ where $a = C/I$.

CASE 1.

A disc of moment of inertia $I$ on a shaft of stiffness $C$ with the other end fixed.

The angular motion is simple harmonic.

$$\therefore\ t = 2\pi\sqrt{\frac{\text{displacement}}{\text{acceleration}}} = 2\pi\sqrt{(\theta/\dot{\omega})} \text{ where } \begin{cases}\theta = \text{angular displacement}\\ \dot{\omega} = \text{angular acceleration}\end{cases}$$

If twist $\theta$ correspond to torque $T$, $\theta = T/C = Tl/nJ$ and $T = I\dot{\omega}$.

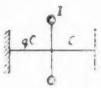
$$\therefore\ 1/t = k/2\pi = (1/2\pi)\sqrt{(C/I)}\quad \therefore\ k = \sqrt{(C/I)} \text{ or } y - 1 = 0 \qquad . \qquad (1)$$

This assumes that the moment of inertia of the shaft is small in comparison with $I$. If this be not the case the shaft inertia may be allowed for by including one third of its value in the value $I$.

CASE 2.

Here $k^2 = qC/I$. $\therefore\ y = q$ or $y - q = 0$

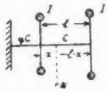
. . . . . (2)




CASE 3.

$k^2 = (qC + C)/I$. $\therefore\ ay = a(q + 1)$. $\therefore\ y - (1 + q) = 0$

. . . . (3)




CASE 4.

Assume node at $N$—a node is equivalent to a fixing.

For R.H.S. we have $k^2 = lC/(l - x)I$. $\therefore\ y = (l - x)$ or $l/x = y/(y - 1)$.

From L.H.S. $k^2 = (qC + lc/x)/I =$ $\therefore y = q + l/x = q + y/(y - 1)$

whence $y^2 - (2 + q)y + q = 0$ . . . . (4)

CASE 5.

Assuming a node at $N$.

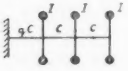
For R.H.S. $k^2 = \{ lc/(k-x)+c \} /I$.

$\therefore\ y = l/(l-x)+1$ which gives $l/x = (y-1)/(y-2)$.

From L.H.S. $k^2 = (qC+lc/x)/I$. $\therefore\ y = q+l/x = q+(y-1)/(y-2)$,

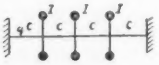
whence $y^2-(3+q)\,y+(1+2q)=0$ . . . . (5)

The following equations have been derived in a similar manner:—



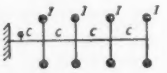
CASE 6.

$$y^3-(4+q)\,y+(3+3q)\,y-q=0 \quad . \quad . \quad (6)$$



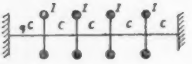
CASE 7.

$$y^3-(5+q)\,y^2+(6+4q)\,y-(1+3q)=0 \quad . \quad (7)$$



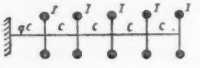
CASE 8.

$$y^4-(6+q)\,y^3+(10+5q)\,y^2-(4+6q)\,y+q=0 \quad (8)$$



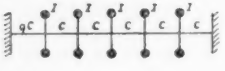
CASE 9.

$$y^4-(7+q)\,y^3+(15+6q)\,y^2-(10+10q)\,y+(1+4q)=0 \quad (9)$$

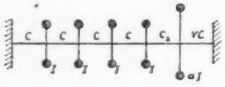


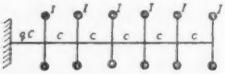
CASE 10.

$$y^5+(8+q)\,y^4+(21+7q)\,y^3-(20+15q)\,y^2 + (5+10q)\,y-q=0 \quad . \quad . \quad (10)$$

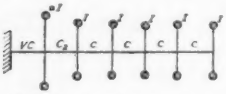


CASE 11.

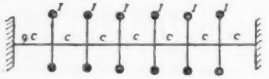
$$y^5-(9+q)\,y^4+(28+8q)\,y^3-(35+21q)\,y^2 + (15+20q)\,y-(1+5q)=0 \quad . \quad . \quad (11)$$

CASE 12.

$$y^3-6y^2+10y-4 = \{ C/C_2-(1/[ay-V]) \}\ (y^4-7y^3+15y^2-10y+1) \quad (12)$$

CASE 13.

$$y^6-(10+q)\,y^5+(36+9q)\,y^4-(56+28q)\,y^3 + (35+35q)\,y^2-(6+15q)\,y+q=0 \quad . \quad (13)$$



CASE 14.

$$y^4-7y^3+15y^2-10y+1 = C/C_2-(1/[ay-V]) \}\ (y^5-8y^4+21y^3-20y^2+5y) \quad (14)$$

CASE 15.

$$y^6-(11+q)\,y^5+(45+10q)\,y^4-(84+30q)\,y^3 + (70+56q)\,y^2-(21+35q)\,y+(1+6q)=0 \quad (15)$$

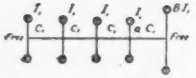
FREE-FREE ENDS.

CASE 16.

$$k_2^2 = C_1/I_2 \quad k_1^2 = C_1/I_1$$

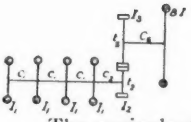
$$k^2 = k_1^2+k_2^2 = C_1\,(1/I_1+1/I_2) \quad . \quad (16)$$

which is the case of the single throw radial engine.



CASE 17.

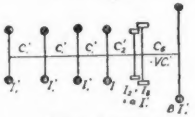
$$y^3-6y^2+10y-4 = (a\beta/[\beta y-a])\ (y^3-5y^2+6y-1) \quad (17)$$



CASE 18.

Where $t_2$ and $t_3$ denote the number of teeth in the pinion and gear wheel respectively.

The equivalent system for the above is as below :—



$$a=(I'_1+I_3)/I'_1 \quad V=C_6/C'_1 \quad C_1/I_1=C'_1/I'_1$$
$$C'_1=(t_3/t_2)^2C_1 \quad C'_2=(t_3/t_2)^2C_2 \quad I'_1=(t_3/t_2)^2I_1$$

The frequency equation is then

$$(1+1/\beta)\,y^3-(5+6/\beta)\,y^2+(6+10/\beta)\,y-(1+4/\beta)$$
$$=\{(C_1/C_2)\,y-(\beta y-V)/(a\beta y-\beta V-aV)\}\,(y^3-6y^2+10y-4) \quad . \quad (18)$$

If $\beta$ is infinitely large, so that $1/\beta=0$, *i.e.*, the airscrew inertia moment is taken as infinite, the equation becomes :—

$$y^3-5y^2+6y-1=\{C'_1/C'_2-1/(ay-V)\}\,(y^4-6y^3+10y^2-4y) \quad . \quad (18a)$$

## DISCUSSION

Colonel SEMPILL : We are all of the opinion that Major Carter has earned our admiration and deserves to be congratulated not only on his excellent Paper but on the concise and clear summary he has given us of it. The Paper is full of interest and also of those higher mathematics which will be appreciated by some here to-night better than others! The audience to-night is a good one considering the highly specialised subject, but there are a number whose work will enable them to contribute very usefully to the discussion. I would remind you that a good discussion is the best tribute we can pay in appreciation of such an excellent lecture. I will now ask Mr. Alan Chorlton, of Messrs. Wm. Beardmore and Co., Ltd., to open the discussion.

Mr. A. E. L. CHORLTON : My connection with torsional vibration began many years ago, so that anything that has taken place in developing a solution of this difficulty is always of interest.

So long ago as 1904 I was concerned with the manufacture of electric generator sets for three-wire systems in which Willans' engines were used for direct driving two generators arranged tandemwise. Here on account of the two heavy armatures and the engine flywheel, we had in effect three flywheels, and considerable trouble was experienced due to resonance, several shafts being ruptured.

This was one of the very early cases of torsional resonance, and naturally the cause of the breakage was at first unknown. Finally Captain Sankey, being then concerned with the Willans' engine, determined the cause in conjunction with Messrs. Chree and Millington, as mentioned by Major Carter.

Mr. Frith, with whom I was associated at that time, demonstrated the action by experiments with glass rods, a method which might be considered by the Author of the Paper.

I very much appreciate the work which has been done by Major Carter, as it has been very valuable indeed, and I should like to congratulate him on his Paper.

What one looks for in a Paper such as this, is for an indication as to how to avoid the stresses indicated for multi-cylinder engines working at high power. Both in Germany and in the United States, engineers have been for some years very concerned about the effects of resonance, and very great attention has been and is still being paid to it, as indicated by the number of papers read.

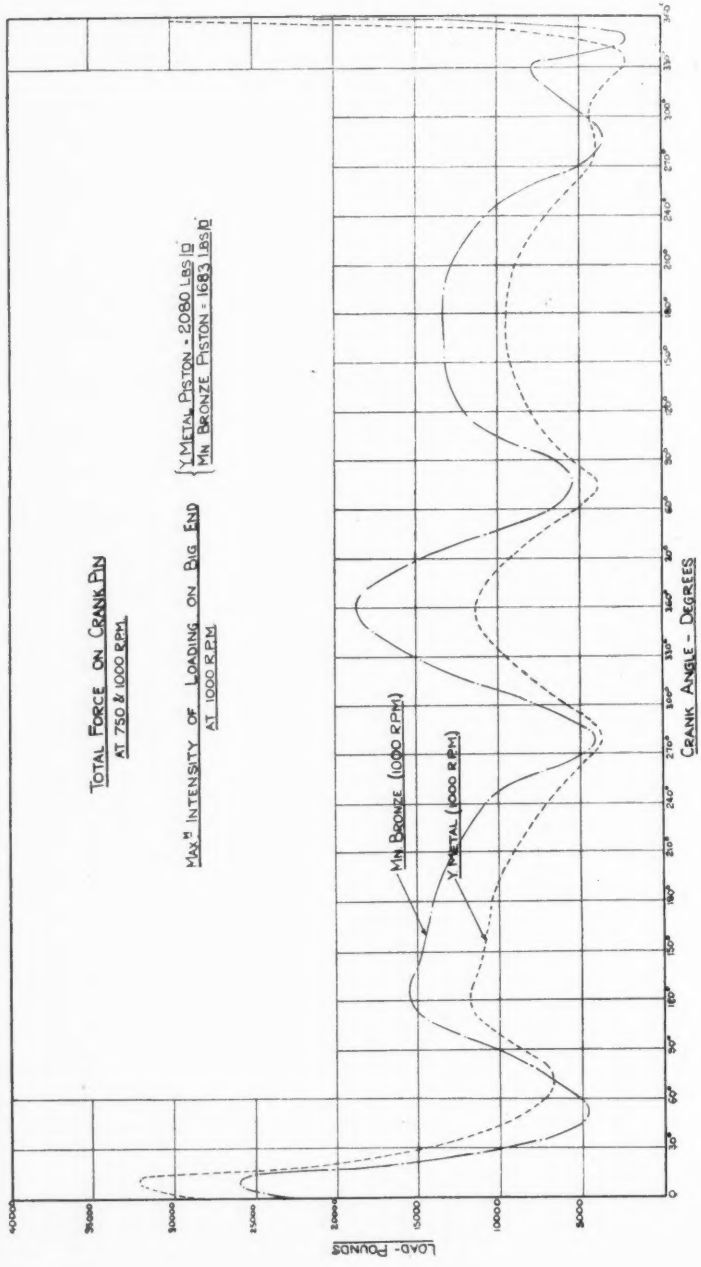

TOTAL FORCE ON CRANK PIN
AT 750 & 1000 R.P.M.
MAX.M INTENSITY OF LOADING ON BIG END
AT 1000 R.P.M.
Y METAL PISTON = 2080 LBS (Q)
MN BRONZE PISTON = 1683 LBS (Q)
MN BRONZE (1000 R.P.M.)
Y METAL (1000 R.P.M.)
LOAD - POUNDS
CRANK ANGLE - DEGREES

FIG. 1.

As high pressure multi-cylinder engines are now being considered for and applied to many transport propositions, it is important to know what can be done to overcome these troubles, as we are bound to go on extending the use of such engines.

In the beginning of his Paper, the Lecturer spoke of one amelioration of the stresses by using the two cycle engine and increasing the weight of the reciprocating parts.

In a paper ("The High Efficiency Oil Engine," Proc. Inst. of Mech. Engrs., 19/3/20) I gave last year, I pointed out that we had considered the latter and for the time being were trying it out by using a manganese bronze piston, which has a material effect in bringing down the peaks (see attached diagram, Fig. No. 1).

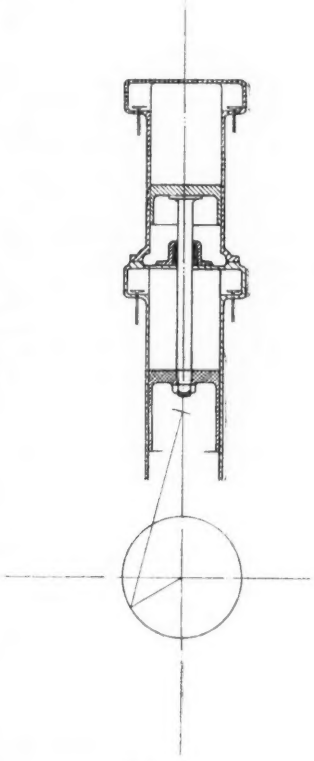


FIG. 2.

With regard to the two-cycle engine, this is a type of engine which always seems to be enticing some of us to consider it as the engine of the future. With this view I can hardly agree, as it does not meet the call for economy necessary in an aero engine of the future. Therefore, if you are going to design a special engine to operate with a high maximum pressure, you should, I think, do it in another way, and I suggest that this can be done in the following manner (see attached diagram No. 2).

I would like Major Carter to consider further and from other angles than

increasing the weight of parts, because this is really rank heresy to make engine parts heavier according to the present school of designers.

There is not a motor engine constructor here who will support Major Carter, I feel sure, in saying that we should make parts heavier.

Motor engine designers invariably make pistons and other moving parts of the very lightest possible materials in order to reduce the weight and obtain the minimum inertia effects.

I should also like him to expand his consideration of the question and possibilities of damping.

A practical point is that if we have to increase the crankshaft diameter in order to augment its strength and put up its period, we should make it of carbon steel as this material relatively has the same stiffness (the property required) and not the highest ultimate strength, and it is preferable, in my opinion, to the high value nickel chrome steels because it is much less difficult to secure sound shafts.

Captain MORRIS: The first thing that struck me about Major Carter's Paper was the absence of that data which Mr. Chorlton referred to. I think to a certain extent I can answer that question because I have done a fair amount of work on the theory and apart from that have had a number of consultations on the subject. Recently an American firm were in difficulties with a 3,000 h.p. Diesel engine on test with a dynamometer. It was a perfectly straightforward case. In front of the engine there was a massive flywheel and between this flywheel and the dynamometer rotor there was some eleven feet of shafting averaging about sixteen inches in diameter. Now where you have a massive flywheel or pulley in a system you can separate out the torsional vibrations on either side of this flywheel or pulley, much in the same way as a long bearing will separate out bending and lateral vibration on either side. So that in this case I calculated the frequency of torsional vibration of the dynamometer rotor on the shafting up to the flywheel—the sort of thing illustrated in Fig. 1 of Major Carter's Paper. Then I found when resonance should occur between this and the frequency of the water shocks set up on the dynamometer.

In view of the massive shafting employed, I also went into the question of its weight. I thought it might have some effect. As a result I produced a simple formula embodying this factor and found that the weight of the shaft had very little influence on the problem.

With the actual data which was ascertained on the job, my formula gave 98 r.p.m. as the resonant speed and that when running the worst vibration was experienced in the region 98-100 r.p.m., so that the agreement between theory and practice was very close.

Now it seems to me that this case was a good test for the theory. I supplied the formula and the firm used it and found it accurate in practice. In the end no alteration was necessary as at the test speed of 90 r.p.m. the vibration was not abnormal.

I am sometimes asked the question, "Supposing you are given drawings and other necessary data, can you say from your theoretical work what is going to happen in so far as critical speeds are concerned?" My answer is in general "Yes."

As an example, I investigated a case of a series of failures of crankshafts of an aero engine on test and was able from the drawings and data to calculate the exact critical speed and the effects of various changes which had been made on both test gear and engine crankshaft.

It is a comparatively easy matter to work out the critical speeds of torsional resonance in an aero engine with or without transmission gear. The most difficult part is not the mathematics but what data to use in the formulæ. The one

factor which took some time to decide on was the "equivalent journal." To regard the ordinary journals alone as taking the torsional strain will in some cases lead to erroneous results. After some tests on the torsion of a crankshaft I have come to the conclusion that in general aero engine crankshafts can be regarded as twisting up in normal running as though the journals were unrestricted. The amount of displacement is well within the play. So that for the equivalent journal, I take the system journal—crankweb—crankpin and obtain the torsional coefficient of this system. With such coefficient I have obtained very close results—by close I mean within 5 per cent. of the actual value.

I consider that having regard to the knowledge available it is largely through carelessness that some designers are troubled with torsional resonance.

(*Communicated.*)—I am glad to hear, however, from Major Carter that designers are now making consideration of resonance effects the starting point of design. This speaks volumes for the progress made in grappling with the problem.

I observe that Major Carter has propounded a thesis to the effect that in the case of aero engines resonance can be converted from "an enemy into a friend." On the basis of his "resonance factor" graph, as in Fig. 2 of his Paper, he has deduced the proposition that by running at a speed in excess of $\sqrt{2} \times$ the resonant speed, the stress in the shaft will be less than if there was no resonant speed below the running speed. It seems to me that it is quite immaterial as to what the "resonance factor" is; what does matter is the actual stress in the shaft for different speeds. If we plot the actual maximum displacements in a shaft against speed we will find that for the kind of forced vibrations with which we have to deal, the resulting graph will portray lessons the very reverse of those inferred by Major Carter from his "resonance factor" picture. As an example, the stress in lateral vibration (with rotation) is least in the range $\Omega^2 = 0$ to $\Omega_c^2/2$ where $\Omega_c$ is the critical speed; it grows from zero at $\Omega^2 = 0$ to a particular value at $\Omega^2 = \Omega_c^2/2$ which is subsequently exceeded and only ultimately equalled for an infinite speed.

There is one observation I wish to make concerning Major Carter's reference to my work on multi-throw engines. He says, my work expresses in "concise form" equations obtained by the methods of Chree, Millington and Sankey for the particular case where "all the throws are equal and the average stiffness between crankpin centres may be taken to apply all along the shaft." In the first place I had never seen or heard of this paper until after the publication of my paper "On the Stability of Aero Engines" in the Journal for April, 1923. This, however, is a minor point. Secondly, my method applies quite generally to cases where the stiffnesses are not necessarily equal, *e.g.*, in the case where the central journal differs from the others in stiffness or where alternate journals only are equal, etc.

The great advance which I claim to have made is in matter of notation. With the notation adopted in my letter published in the Journal for October, 1923, it is possible to do a calculation in a few minutes which on the lines of Chree, Millington and Sankey would take as many days or weeks, according as the number of throws increases.

This sort of thing is well described in an article on the teaching of mathematics, by Mr. T. J. Garstang, in a book "The Public Schools from Within" (Sampson Low, 1906). "Questions in arithmetic that are now easy to a small schoolboy armed with pen and ink and the decimal point, would have puzzled the ablest man in Plato's school before the Arabs had brought their figures to Western lands. De Morgan, in the nineteenth century, took less than ten minutes to find a cube root so difficult that in the seventeenth century the most skilful calculator would have required a month. Moreover, the immortal Gauss

finished by superior notation in one hour a calculation which had taken Euler three days and left him blind."

I should like to finish up with a tribute to Major Carter for the persistency with which he has pursued this very valuable work, and for presenting to us through the medium of the Society this most useful Paper.

Mr. A. C. LOVESEY: My experience has been wholly confined to multi-crank engines and I have found that critical speeds could be estimated within about 7 per cent. on such engines from a knowledge of the crankshaft stiffness and polar moment of inertia of the parts, the values for stiffness and inertia being determined experimentally.

It was further found that assuming half the mass of the pistons concentrated at the respective crankpins and including this in the inertia of the crankshaft, gave good results, where previous calculation neglecting the piston masses showed that the calculated values fell short of the observed.

Reference was made to the fact that tightening up the airscrew raised the critical speed to a higher value. It would be interesting to know if the energy dissipated in friction by the loose airscrew—as shown by the hysteresis curves—resulted in any appreciable reduction in the amplitude of the vibration at the resulting critical speed.

The Austro Daimler Co., I believe, employ their flywheel frictionally mounted, presumably as a means of avoiding torsional resonance within the working speed range. We once carried out a similar experiment in which the flywheel was frictionally clamped to the crankshaft and could slip a few degrees relative to the crankshaft against this friction before its movement was restricted by rubber buffers. This resulted in removing the critical speed from the working range, but a good deal of "thrashing" of the flywheels against the rubber buffers was evident at low speeds, due to the engine torque fluctuation.

I note from Diagram 8 that when the flexibility of the shaft was increased to lower the synchronous speed from 1,400 to 820 r.p.m., the torque peak of the vibration was considerably reduced. I understand that the "resonance factor" is the same at each of these speeds and that this reduction of vibration amplitude is due to the smaller gas torque at the lower speed. This being the case, it appears that no advantage could be gained by application of the same method to an automobile where the gas torque can remain fairly constant and even become higher at the low speeds.

Some experiments were made with a solid friction damper consisting of a small flywheel at the front end of a six-throw crankshaft and free to slip torsionally against the friction. A point which was found to differ from Major Carter's observations on radial engines, was that the damper did not lower the critical speed, but resulted in a reduction of the amplitude of the "master" vibration and the growth of another vibration at a lower speed which was previously free from vibration.

By further increasing the damper friction, the vibration at the original critical speed could be reduced, but at the expense of increasing the vibration at the lower speed. With the damper clamped solid the whole vibration was transferred to this lower speed.

It was further found that the lower speed vibration in all its stages was much more critical than the higher one requiring only a slight variation of speed to pass through it. The high speed vibration extended over a much greater speed range.

These results seem more what would be expected from a flexibly driven damper. I should be interested to know if this result is considered due in some way to the use of a solid friction damper as opposed to the viscous friction type

considered in this Paper or, being at the end of the six-throw crankshaft, it approximated to the case of a flexibly driven damper.

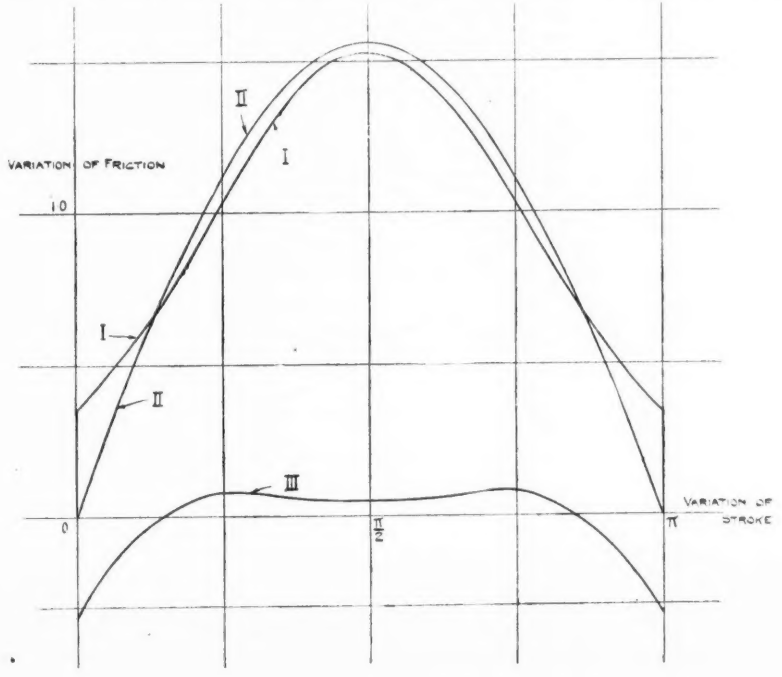
With a six-crank engine not fitted with any form of damper one is often troubled with the effects of the harmonics of the gas torque curve which appear at $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, etc., of the main synchronous speed when these speeds are encountered with large throttle openings. Usually the main synchronous speed is well outside the working range and it is only these lower speed torsional vibrations which are encountered.

We have found that a solid friction damper, without being unduly heavy, is very effective in removing these vibrations, although the same damper has little effect on the main synchronous vibration which however comes well outside the normal speed range of the engine.

Captain SAYERS: I should like to ask whether in the case of a single crank radial the attempt has been made to reduce the stiffness of the shaft so that the synchronous speed comes well below the running speed, so that in working conditions the resonance factor reduces shaft stresses and whether the results have been satisfactory?

Major LOW: He would confine his remarks to Major Carter's simplifying assumption that friction was proportional to velocity, an assumption made by most writers, as it led to simultaneous linear differential equations with constant coefficients for which there was a straightforward solution.

Now a system in motion, under frictional forces only, came to rest in a time which was finite, logarithmically infinite or algebraically infinite, according as the friction was proportional to a power of the velocity less than, equal to or greater than unity, and this no doubt suggested to critics the assumption that



there might be an equally complete change in the type of motion discussed in the Paper.

In the absence of experimental data as to the real distribution of the friction forces over these three types, one third of the total dissipation is assigned to each and the resulting distribution, in space, along a stroke, is shown in the figure as curve (I.).

$F_1 = \{ \frac{1}{8} + (\pi/6) \sin \theta \} + (\frac{2}{3}) \sin^2 \theta$ in comparison with the simple sine curve (II.) of the same area $F_2 = \frac{1}{2}\pi \sin \theta$.

The difference of ordinates gives curve (III.) showing the comparatively small forces which would have to be impressed at suitable points in the system, in order to make the facts agree with Major Carter's assumption.

The residual problem lies in the effects of these neglected forces which may conceivably set up troublesome resonance with appropriate natural periods, even when Major Carter's solution indicates freedom from resonance with the fundamental component ($F_2$).

No doubt if and when such troubles arise Major Carter will be able to deal with them as successfully as he has dealt with the main problem.

He added his appreciation of the work, in that he had had the privilege of studying the Lecturer's mathematical work, and apart from minor suggestions as to notation had not been able to make any significant improvement in the worked out analysis.

Mr. BRAMSON: Major Low raised the question of higher harmonics. I should like to ask the Lecturer if any of the higher harmonics have been known to become of sufficient amplitude, or sufficiently in resonance to be harmful. It strikes an outsider to the problem that the net result of most of the arguments we have heard is that in trying to avoid this trouble of crankshaft vibration one has got to increase the flexibility of the shaft in relation to its inertia in order to get the frequency of resonance down to such a value that it corresponds to a low torque, and therefore a low torque variation. I should like to know whether that accurately summarises the position. If it does it strikes one that it is not the first case in which increase of flexibility has cured the trouble. In the De Laval turbine the spindle is extremely thin and flexible, so as to lower the whirling speed. Propeller blades are elastic and will bend in the plane of rotation when transmitting angular accelerations to their own tips. That may possibly account for the fact that the curves generally seem to show a higher rigidity arrived at by calculation than by experiment.

One wonders whether the play in big-end bearings has the effect of damping or flexibility. I can suggest one source of absorption of energy, namely, waves and turbulence in the gases contained in the cylinders.

I think this subject has a bearing on another form of resonance which sometimes takes the form of wing flutter. I should like to know Major Carter's opinion as to whether any of the engine frequencies are in practice likely to be in resonance with any of the structural frequencies of aeroplanes.

Mr. FEARN: Primarily I should like to refer to the matter of viscous damping which was mentioned by the last speaker.

To do this let us refer to the elementary case shown in Fig. 1 of the Paper and let $\theta$ be the angular movement from its normal position of any diametral line scribed on the flywheel, then the motion can be represented more completely by the equation:—

$$I\ddot{\theta}/g + \lambda\dot{\theta} + c\theta = 0$$

where $\lambda$ is the coefficient of damping due to viscous resistances. Solving this equation we obtain the value:—

$$\theta = Ae^{-\lambda t g/2I} \sin (qt + \alpha) \quad . \quad . \quad . \quad . \quad . \quad . \quad (A)$$

where $q = (4I_c/g - \lambda^2)^{\frac{1}{2}}/(2I/g)$ and $A$ and $\alpha$ are arbitrary constants.

If we make $\lambda=0$, then $\theta=A\sin[(cq/I)^{\frac{1}{2}}.t+\alpha]$, and hence the frequency $n=(1/2\pi)(cq/I)^{\frac{1}{2}}$, which is, of course, Major Carter's original result. Comparing this result with the value obtainable from equation (A) we see that the frequency is affected by the viscous damping which also adds another motion of amplitude $Ae^{-\lambda t g/2I}$.

Useful results might be obtained by a development of this idea and I think that you will find that the phase change due to damping is negligible at all speeds excepting the critical speed. It is only at this critical speed that the damping need come into consideration; its effect being to impose a restraint and so prevent excessive angular amplitude of the shaft.

*Reciprocating Masses.*—On page 295, paragraph 3, is given a method of dealing with these masses. In the past it has been my practice to replace the reciprocating masses by a solid flywheel. The justification for this assumption is as follows:—

The displacement of the piston from the top dead centre $x=R+L-R\cos\theta-L\cos\phi$.

$$\therefore\ \dot{x}=R\sin\theta\dot{\theta}+L\sin\phi\dot{\phi}$$

$$\dot{x}=R\sin(\dot{\theta}+\dot{\phi})$$

Hence with a connecting rod of infinite length

$$\dot{x}=R\sin\theta.\dot{\theta}.$$

If $M$ be the mass of the reciprocating parts then their kinetic energy $T=\frac{1}{2}M\dot{x}^2$.

$$\therefore\ T=\tfrac{1}{2}MR^2\sin^2\theta.\dot{\theta}^2.$$

$$\text{Average value of } \sin^2\theta=(1/2\pi)\int_0^{2\pi}\sin^2\theta d\theta=\tfrac{1}{2}.$$

$\therefore$ Mean value of K.E. $=\bar{T}=\frac{1}{2}(MR^2/2)\dot{\theta}^2$, which agrees, Sir, with your result. Hence the reciprocating masses can be treated as a *solid* flywheel of the same radius as the crank if we assume that the connecting rod be of infinite length. For this reason one can consider the auxiliary rod in the same manner, since if it be justifiable to neglect "obliquity" effects for the master rod we may neglect with impunity any increase in this effect consequent to the use of an auxiliary rod.

Under the heading "Driven Auxiliaries," on page 295, it is stated that "the stiffness" (*i.e.*, of a crank) "may be found by a process of calculation and judicious guess work."

For the benefit of those who have not had Major Carter's experience in dealing with this matter, perhaps he will give us further aids so that we may all "guess judiciously." For example, is it considered that the crankpin is subjected to a twisting moment? If there were no radial clearance in the bearings then obviously the torque would be transmitted through the crankpin by a shearing force only and there would be no twisting moment at all. Is the normal bearing clearance used in aero engines sufficient to vitiate this argument? A reference has been made during the evening to the "equivalent" journal. Would Major Carter consider the replacement (for stiffness purposes) of the webs and crankpin by a continuation of the journal for a similar length to be of sufficient accuracy? If Major Carter will elucidate these points for me I shall be extremely grateful.

Mr. J. S. LAWSON OSWALD: I am sorry that the Lecturer has not had time this evening to cover that part of his lecture which deals with "Reaction on Engine Mountings," a subject of vital interest to the aeroplane designer.

A very well known engine firm supplies with its standard data the torque variation and the polar moment of inertia of the complete engine. The reason given for supplying this data is in order to estimate—

(1) The amount of damping out of the engine torque variation.
(2) The effect of any possible synchronisation of the natural torsional period of the engine mounting with the engine explosion period.

If the firm supplied the polar moment of inertia of the crankshaft and its attached masses, such as has been repeatedly used by the Lecturer this evening, one could see the value of such a polar moment of inertia for the above estimation as indicated by the Lecturer this evening. My point being that the cylinders, etc., should not be included in the polar moment of inertia supplied.

One is, of course, only dealing with the effect on engine mountings and not questioning the value of the polar moment of inertia of the complete engine in the calculations which may be necessary for certain manœuvres. There is a good deal of dubiety and argument on the above subject among various members of aeroplane design staffs, so that if the Lecturer can clear the matter up it will be of great service to us.

Mr. Wm. D. Douglas: The tracing out of vibration and its experimental measurement is always interesting experimentally and is important in connection with several phases of aircraft performance. There are the comparatively simple lateral vibrations due to out-of-balance of rotating parts; torsional vibrations, such as form the subject of Major Carter's Paper; torsional vibrations of streamline wires and the alternating motions which sometimes occur in wings during flight and are known generally as "flutter." These are all different forms of vibration, but various experimental methods are necessitated for their separate study and measurement.

Torsional vibrations of engine crankshafts do not necessarily produce marked effects outside the engine. "Rough" running of the engine may be noticed, but general severe vibration of the engine mounting may be absent unless the torsional vibration is accompanied by a lateral vibration. It is, therefore, unlikely that torsional vibration only in the engine crankshaft will give rise to any marked resonance effects in the main portions of the aircraft structure.

The recording of torsional vibrations of the crankshaft of an air-cooled engine running at full load involves many experimental difficulties. An air brake or fan, somewhat similar to a propeller, must be used to provide the air stream for cooling and the crankshaft can only be reached from in front of this fan. Fortunately the crankshafts of most of the air-cooled radial engines are hollow as otherwise it would be very difficult to detect or measure any high frequency torsional vibrations which took place.

Mr. T. J. Gerard: The R.A.E. Mark II. type of torsiograph described in this paper and illustrated in Fig. 4A, is of especial interest nowadays when nearly all power transmitting shafts are made hollow for lightness, ease of inspection, or other reasons. Unlike the Hopkinson-Thring type of optical torsiograph, which was designed for use on shafts transmitting a steady torque and provided only for torque measurement at one or two isolated angular positions of the shaft, this instrument gives a continuous record of torque and renders possible the experimental investigation of cyclic torsional fluctuation of shafts.

This instrument is readily adaptable to practically all sizes of hollow shafts. The magnification of the twist of the shaft is partly mechanical and partly optical, the former being dependent on the acting length of the instrument and on the ratio of the distance of the slot "E" from the axis of rotation of the shaft to the length of the tailpiece "D" of the mirror "C," and the latter being dependent on the focal length of the mirrors. Independent adjustment of both the mirrors about vertical and horizontal axes normal to the axis of shaft rotation

facilitates the initial setting of the instrument, and the rigidity of the mirror mounting eliminates change of this initial setting, whilst the instrument is in use.

Wing Commander T. R. CAVE-BROWNE-CAVE: We are grateful to Major Carter not only for his very useful paper but for the important investigation, much of which is his own original work.

He deals chiefly with torsional resonance, an extraordinarily difficult disease and one astonishingly fatal in its effects. I don't agree with Captain Morris that no designers pay any attention to it. Many marine engine designers regard it as the consideration which is first dealt with in the design of a shaft. No doubt some designers take no special precautions until they meet the trouble for the first time, but the consequences of the first attack are usually so serious that the designer never forgets them. The work now published will allow well informed designers in future to avoid the costly experience of the first attack.

A remark at the beginning of the Paper connects these troubles specially with compression ignition engines. It is made, apparently, as an additional justification for the Paper. Almost the whole of the Paper deals with petrol engines. I do not agree that C.I. engines, or even those with few large cylinders, are peculiarly subject to these troubles. The accusation is unfortunate, particularly at a time when it is important not to frighten the designers of light C.I. engines. The Paper is quite sufficiently valuable to stand by itself and I hope that if you presently express your appreciation adequately, Major Carter will leave out the sentence altogether.

The solid friction damper is, for practical considerations, far better to use than that depending on viscous friction. The latter is easier to treat mathematically, but the Author has shown the serious consequence of a change in viscosity no greater than might well take place from a change in oil temperature under running conditions. Major Carter has, I know, connected the performance of the two types and it would be valuable if he would describe how best to deduce the effect of a solid friction type. The description of the work done on solid friction dampers by Messrs. Rolls-Royce, and particularly that of the spring controlled flywheel with solid friction damping, is specially interesting.

The Author refers to the " waggle " of the balance weights. This horrible term fails to convey to me whether the vibration is about the axis of the crankshaft or about another axis.

The really important requirement is to be able to calculate from the drawings of a proposed shaft what its dangerous periods will be. The most difficult step in dealing with a crankshaft is to determine the rigidity of the shaft. This is particularly troublesome in crankshafts which are to be made as short as possible and the thickness of the webs, parallel to the axis of the shaft, is therefore small by comparison to their width. Some speakers have said that this calculation is quite easy, others that it is extremely difficult. Personally, I agree with the latter, particularly as the former do not publish their methods. The incautious designer, if he considers resonance at all, only determines this rigidity factor when the shaft and its housings are made. If an alteration is necessary it must probably be so drastic as to involve almost complete redesign.

I suggest that a model crankshaft made and tested in torsion would give results which by very simple similarity calculations could be converted to represent the full-scale shaft. Alternatively a comparatively cheap research with model shafts might yield data which, if published, would allow designers to calculate their shaft rigidity accurately at nominal expense.

To deduce the period of a shaft from an experimental determination of the resonant period of a model system is more difficult and probably far less reliable.

The rigidity of the crankcase in which the shaft is supported is a factor which was discussed in a recent shaft investigation. I am of opinion that the case may be assumed rigid so far as its effect on shaft resonance is concerned, but I hope Major Carter will give his conclusion on this point.

I was very surprised at Captain Morris's description of a shaft which on trial and according to calculation had a resonant speed at 72 revs./min. The agreement between calculation and fact is not surprising in such a very simple case. It is, however, remarkable that the trouble should have been cured as he says by an alteration of speed of only 4 revs./min., whereas Major Carter's curves show that 25 or 30 per cent. is necessary to reduce the torque fluctuation even to double its normal value.

I hope you will express your gratitude to Major Carter for his Paper and your appreciation of the work which it describes.

Mr. A. H. R. FEDDEN (*communicated*): I much regret that I was unable to be present at Major Carter's lecture as the subject dealt with is of special interest in connection with the design of radial engines. A knowledge of the dynamic characteristics of a proposed engine is essential if the failure of important parts is to be avoided, and we have followed the enunciation of the principles governing the prediction of such characteristics with the keenest interest.

During the past few years it has been our practice to stress the main moving parts of a new engine primarily from the dynamic standpoint, *i.e.*, the effects of vibration in increasing the range of stress are taken into account. These calculations precede the detail design of the part concerned instead of following it, and it is interesting to note that since these methods were adopted, no failure of any major part has occurred.

It is, of course, highly desirable that experimental verification should be obtained for any calculations such as these, as several doubtful assumptions are involved, and the particulars given by Major Carter of stiffness tests will be generally welcomed. Quite apart from the somewhat involved mathematics necessary to deal with the crankshaft system, there are points not amenable to calculation—*e.g.*, propellor hub and spline connections—about which the experimental data quoted will prove of the greatest value.

We heartily agree with him, however, when he says that the calculation of shaft stiffness is by no means an exact science. The torsional and bending stiffness of the front crank web, and the amount of support afforded by the rear web and the rear main bearing, are points on which more light is needed. We have, it is true, evolved a rational (as opposed to empirical) treatment of this section, but it must be confessed that reference to an existing engine was necessary for some of the basic assumptions involved.

The section of the paper dealing with supercharger drives and their effect on the synchronous speed gives rise to some interesting speculations. For instance, in an engine fitted with a geared blower, the equivalent inertia of the blower is extremely high, and it appears at first sight, that this could be used to remove the synchronous speeds of the whole system to any desired region, and it is very unfortunate that, as shown on page 299 of the paper, this necessitates making the blower drive of the same order of strength as the airscrew drive.

Possibly, as indicated in Case 3, page 300, an elastic damper drive would help, and further work in this direction should be very valuable.

## REPLY TO DISCUSSION

The investigation of torsional resonance in high speed engines has received help in this country from the classical work done by Messrs. Chree, Millington and Sankey, referred to by Mr. Chorlton, and from the scientific work of mathematical physicists, amongst whom may be mentioned the late Lord Rayleigh,

who investigated the fundamental nodes of vibration of bodies of all kinds and evolved general methods of determining frequencies.

The former work relates largely to flywheels connected to relatively light shafting and the latter covers the field of vibrating rods, bridges, etc., of complex and varying form.

Crankshaft systems may be treated from either standpoint and concordant results obtained. This is a reassuring fact to those who have worked only upon the flywheel shaft analogy and have felt that this analogy is rather strained in the case of multi-crank aircraft engines. This matter will be returned to later.

Much can be learnt by pondering over models and Mr. Chorlton's suggestion that models might be made of glass is interesting. For quantitative investigation there is the possibility of making steel models, a matter raised by Commander Cave. I am acquainted with a case where a model was made of a crankshaft of a large marine Diesel engine in order to determine the stiffness which, incidentally, proved to be equal to that of a plain shaft of the same overall length and of the same diameter and bore as the journals. I doubt whether the same procedure is applicable in respect of aircraft engines because errors in manufacture in a small model, delicacy of the tests, and the question whether bearing clearances and rigidity of bearing supports for the model are suitable would tend to make the results inferred for the main shaft insufficiently reliable.

Mr. Chorlton has raised some general questions which are difficult to answer satisfactorily in a brief reply. I am afraid I did not make my observations clear about offsetting gas and inertia forces. I was not advocating increasing the weight of reciprocating parts this end, but had in mind the investigation of particular types for Diesel work such as the opposed piston and the two-stroke radial types. In Fig. 15 there is shown a diagram of loadings of the big-end of the "Dragonfly" engine. If this engine were converted to two-stroke operation gas pressures would oppose inertia as the crank passed *each* cylinder instead of *alternative* cylinders and the big-end loadings would be more uniform and less in relation to the power output; thus, so far as the crankpin is concerned, inertia forces would be offset against gas forces with resultant reduction of bearing loading.

I agree with Mr. Chorlton that, although maximum loadings might be reduced in a particular engine by increasing piston mass, the step is not one which commends itself. Added weight may be important, the mean loadings may be increased and the unbalance of the engine increased, also the inertia reactions within the engine will be made greater; furthermore, the synchronous speed will be lowered, and this is objectionable where the design is based upon the principle of keeping the main synchronous speed beyond the maximum running speed. In the opposed piston type, if the inertia force of each piston be made equal to one half the maximum gas force, the load coming upon the big-end will be about half that due to the gas pressure, *i.e.*, equal to the inertia force at each end of the stroke, and high maximum loads will be avoided.

As regards Mr. Chorlton's tandem engine arrangement, he does not show how he proposes to build such units into complete engines. For normal types of engine installation the distance from crankpin centre to top of cylinder would be against such a scheme. The combustion chamber form for compression ignition work in particular cannot be made very good in the lower cylinder with valves at the side; and then there is the gland which requires to be made mechanically satisfactory.

Mr. Chorlton has asked me to expand my remarks upon damping. I am not acquainted with any case of an aircraft engine having been fitted with a damper to overcome serious torsional vibration and can thus only remark upon the matter from experience with other types of engines and from theory. To

control main torsional vibration, the size of damper and strength of drive required are likely to prove excessive. A damper of moderate size suffices to remove vibrations at sub-multiples of the main synchronous speed due to harmonics of the torque impulses, as Mr. Lovesey has stated. In the case of single throw radial engines a smaller damper should be more effective when its drive has suitable flexibility than when the drive is rigid, and possibly the same applies to multi-crank engines. It is difficult to generalise as the possibilities of successful application must be investigated for each case, taking into consideration the position of the main synchronous speed in relation to the running speed. The final point raised by Mr. Chorlton about using carbon steel for crankshafts is a matter to be decided by design requirements of strength and stiffness. One can imagine cases in which stiffness is the ruling factor and the stress is such that carbon steel is to be preferred, but it is to be remembered that when scantlings are increased to keep the synchronous speed above the maximum speed the tendency will be to have a narrow margin and the resonance effect at full speed may be such as to make the stresses high in spite of the increased scantlings. It may be found more satisfactory to keep the main synchronous speed low and work beyond it, keeping the scantlings small and using alloy steels.

As regards converting resonance from being an enemy into a friend, it is evident that I have not succeeded in making myself understood by Captain Morris. I have endeavoured to discover wherein his difficulty lies, but with doubtful success. Perhaps the following brief remarks will suffice to make my view of the matter clear. Imagine that a design is being evolved for, say, a single throw radial engine, and that all the main quantities and scantlings have been fixed provisionally. A designer who is disregarding torsional resonance effects will work out a gas torque curve, inter alia, and calculate torsional stresses and their variations from this. A designer who takes torsional resonance into account will design a shaft for stiffness as well as for strength so as to give the main synchronous speed the most satisfactory value practicable. He may expect to find that the main synchronous speed cannot be put very far beyond the maximum running speed nor brought well below the lowest full throttle speed without introducing novel or undesirable design features such as too long or too short a shaft. For each extreme case he can calculate the resonance factor and, knowing the fundamental gas torque variation at full throttle, he can determine the actual torque variation in the shaft by applying the resonance factor, the smaller this factor the better. Theory shows that the resonance factor cannot be less than unity if the main synchronous speed is beyond the mean running speed range, but may be much less than unity in the other case, which means that the torque variation the shaft must be designed to withstand may be reduced by resonance effects by having the main synchronous speed well below full throttle speed range. Captain Sayers inquired whether this idea has been proved out in an actual engine. A convincing proof involves taking torsiograph records from the shaft of an appropriate engine and this has not been done so far. Of the issue there can be no reasonable doubt, and one has general experience which goes far towards confirmation, but in any one case other design considerations may obstruct the adoption of a low main synchronous speed. For multi-crank engines the matter is not so simple, as torsional vibration may occur with more than one node.

The point mentioned by Mr. Lovesey about car engines having to run sometimes at full throttle at low speeds is important. It causes car engine designers to make their crankshafts stiff enough to put the main synchronous speed well beyond maximum speed. In the case of aircraft engines, however, the airscrew serves to interconnect speed and torque, so that the torque is low at low speeds, and this makes feasible a different basis of design as regards synchronous speed. Where a variable pitch airscrew is fitted, the throttle speed relationship is nearer to that obtaining for car engines that when the engine is connected to a feasible. It is to be remembered and a low main synchronous speed may not be

dynamometer-brake, the throttle speed relationship depends upon the brake setting, and with a low synchronous speed engine precautions need to be taken in running through the critical speed range. According to the test bed arrangements, however, the critical speed on the bed differs from that for the engine fitted with an airscrew. This is an important point which is not always fully appreciated.

As regards Captain Morris's concluding remarks, the difficulty of computing synchronous speed from algebraic formula can be much reduced by adopting graphical methods, as will be gathered by reference to pages 307-309, containing matter not given in the advance copies of this lecture.

For example, the curves of Fig. 14 could have been calculated from algebraic formulæ without undue difficulty, and having been obtained they are available for reading off the solution of each problem as it arises when $q$, $C$ and $I$ have been determined.

To calculate the lowest frequency for a multi-crank engine, I understand that Mr. Lovesey takes, as an equivalent system, a mass of one third the total crankshaft polar inertia moment (including half the reciprocating masses), attached to a light shaft of stiffness equal to the overall stiffness of the crankshaft.

By this method the lowest frequency for $n$ cranks is

$$(1/2\pi)\,(C/I)^{\frac{1}{2}}\,[3/n\,(n-1+1/q)]^{\frac{1}{2}}$$

taking $\sin(\alpha/2)$ or $\frac{1}{2}\sqrt{y}$ to be $\frac{1}{2}\,[3/n\,(n-1+1/q)]^{\frac{1}{2}}$

if, for example, $n=6$ and $q=1$, it is the same as taking $\sin\alpha/2=0.144$, whereas the value given by Fig. 14 is 0.1205. Now the foregoing is based upon the crankshaft being assumed equivalent to a uniform heavy rod vibrating in torsion with amplitude of movement proportional to the distance from the fixed end, and it gives a frequency value, for the case taken, 20 per cent. in excess of that given by the flywheel equations. In view of this difference it is well to investigate the matter by Lord Rayleigh's method which may be described briefly thus:— Let there be a system of $n$ inertia masses $I_1$, $I_2 \ldots I_n$, coupled elastically in series by members of stiffness, $C_1$, $C_2$, $C_3 \ldots C_n$, one end of $C$ being attached to an infinite mass. Imagine the system to be vibrating freely in such a manner that $x_1$, $x_2$, $x_3$, etc., denote the amplitudes of vibration of the respective masses; then by equating total strain energy at the extremities of motion to total kinetic energy when the masses are moving at maximum velocity with the shaft unstrained, it can be shown that

$$\text{Frequency} = (1/2\pi)\left[\,\{\,C_1x_1^{\,2}+C_2\,(x_2-x_1)^2+\ldots C_n\,(x_n-x_{n-1})^2\,\}^{\frac{1}{2}}\right]/(I_1x_1^{\,2}+I_2x_2^{\,2}+\ldots+I_nx_n^{\,2})^{\frac{1}{2}}$$

and to get the lowest frequency suitable values require to be given to $x_1$, $x_2$, $x_3$, etc.

Applying this to the engine problem, for lowest frequency of vibration we may take the amplitude of movement of each crank to be proportional to the $r$th power of the flexibility between this crank and the airscrew or flywheel, and then various values of $r$ can be tried to determine what value gives the lowest frequency, $r$ being merely an arbitrary constant.

Putting $qC$ for $C_1$ and $C$ for $C_2$, $C_3$, etc., and putting $I_1=I_2=I_3$, etc., we get

$$\text{Frequency} = (1/2\pi)\,[(C/I)\,\{\,(1/q)^{2r-1}+[(1/q+1)^r-(1/q)^r]^2\ +\ldots$$
$$+\,\{\,(1/q+n-1)^r-(1/q+n-2)^r\,\}^2]^{\frac{1}{2}}/[(1/q)^{2r}+(1/q+1)^{2r}+\ldots+(1/q+n-1)^{2r}]^{\frac{1}{2}}$$

and in the particular case when $q=1$ and $n=6$, this becomes

$$\text{Frequency} = (1/2\pi)\,(C/I)^{\frac{1}{2}}\,\{\,1+(2^r-1)^2+(3^r-2^r)^2+(4^r-3^r)^2+(5^r-4^r)^2$$
$$+\,(6^r-5^r)^2\,\}^{\frac{1}{2}}/(1+2^{2r}+3^{2r}+4^{2r}+5^{2r}+6^{2r})^{\frac{1}{2}}$$

whence the following results can be computed:—

| Value of $r$. | ·5 | ·7 | ·75 | .8 | ·9 | .10 |
|---|---|---|---|---|---|---|
| Frequency $=(I/\pi)\sqrt{(C/I)}$ times | 0.1311 | 0.124 | 0.1233 | 0.124 | 0.125 | 0.1285 |

so that the lowest frequency agrees well with Fig. 14 and for it $r=0.75$, the significance of which is that amplitude of torque variation between the throws for lowest frequency *natural* vibration decreases progressively towards the tail end of shaft. In this case, the torque variation is twice as great between the airscrew and the first throw as between the last two throws at the tail end since we have:—

| | | 1st Ck. | 2nd Ck. | 3rd Ck. | 4th Ck. | 5th Ck. | 6th Ck. |
|---|---|---|---|---|---|---|---|
| | o | $1^{.75}$ | $2^{.75}$ | $3^{.75}$ | $4^{.75}$ | $5^{.75}$ | $6^{.75}$ |
| Amplitude of Swing | = o | 1 | 1.682 | 2.28 | 2.83 | 3·34 | 3·84 |
| Twist Difference | = | 1 | .682 | ·598 | ·55 | ·51 | ·50 |

There are thus two reasons for considering Mr. Lovesey's method to be of limited application, (1) that it leaves the value of $q$ out of account, and (2) that it assumes the amplitude of swing of each crank to be proportional to the flexibility from flywheel to crank, which is not the mode of vibration for lowest frequency for the flywheel system. The fact that he gets within 7 per cent. of agreement between calculation and observation may be attributable to the finite inertia of the flywheel combined with the stiffness of the shaft coupling to the dynamometer resulting in a higher frequency than corresponds to an infinite flywheel.

The deliberate employment of a friction drive to overcome vibration troubles, does not commend itself to me for aircraft-engine purposes. The friction setting would need to be maintained within narrow limits at an appropriate value, and if the device were called into operation for prolonged periods the power dissipated would probably be excessive and likely to produce overheating and deterioration of the friction surfaces; also the weight involved would be against its adoption. The device might however serve in some cases for eliminating risks in running through critical speeds. With a large number of cylinders as often adopted for aircraft-engines, and with small throttle opening at low speeds, the thrashing difficulty mentioned might not be important.

When serious torsional resonance occurs with an engine fitted with a wooden airscrew the effects are likely to prove more than the hub grip upon the airscrew can withstand, in which case slipping occurs and introduces a damping effect which delays disaster to the shaft, but at the expense of charring the airscrew and perhaps setting fire to it. With a better grip, the airscrew may fail by splitting at the tips, if shaft failure does not intervene.

Returning to the subject of dampers at the tail ends of crankshafts, the phenomena Mr. Lovesey describes is largely in accordance with Fig. 11, for the amplitude of the " master " vibration (*i.e.*, where $x=1$) decreases continuously as the damper friction is increased and at the lower speed ($x=$about 0.7) the amplitude increases continuously.

Damping in the engine is not taken into account in Fig. 11. With a damping coefficient $\Delta=0.2$ the intersection point is lowered from $(\theta)/E=3$ to about 2, and the vibration under this condition may not be noticeable. I think, however, that the damper being on a multi-crank shaft causes it to be in effect elastically-driven, and this may bring the valley between the two peaks into the region of $|\theta|/E=$unity.

I think that the foregoing, rather than the use of a solid friction damper, explains the non-existence of vibrations at the intermediate speeds.

Major Low's remarks upon the effects of assuming different laws of damping friction are reassuring, indicating as they do that the assumption of the viscous friction law for the sake of mathematical simplicity is not likely to have led to erroneous ideas upon the matter in hand. Whether or not the neglected forces may set up troublesome resonance effects is difficult to judge, but it is well to bear these forces in mind as investigations proceed. Hydraulic types of damper have been proposed with the definite object of getting square law damping. A method of correlating solid friction and viscous friction dampers is given in R. & M. 1053, and it would take too much space to say much about the matter here. There is one point which should be mentioned, however, namely, that I make the equivalent constant friction damper setting to be $\pi/4$ times the maximum corresponding viscous friction damper torque, whereas Major Low's diagrams appear to be based upon a ratio of $2/\pi$. A few further remarks on the subject of solid friction dampers are given later.

Mr. Bramson inquired whether higher harmonics have been known to give trouble. I am acquainted with the case of an aircraft engine which had a noticeable vibration at one half the main synchronous speed, but this occurred at low throttle opening and had no harmful effects as far as is known. One must distinguish clearly between modes of vibration, and harmonics of the engine impulses. In adopting a flexible airscrew drive with a multi-crank engine, the simplest form of vibration (*i.e.*, with one node near the airscrew) is caused to occur at a low engine speed and will be forced by impulses which at equal time intervals tend to turn, in the same sense, the cranks on which they operate. The next simplest form of vibration (*i.e.*, with one node near the airscrew and the other beyond the crankshaft centre), will occur at some higher speed and will be forced by those impulses which at equal time intervals tend to turn the two portions of the shaft on either side of the second node, in opposite sense. I am acquainted with cases of crankshaft failure in which fracture occurred with several shafts towards the tail end under conditions which indicate two-node vibration as the cause, but at present this is little more than surmise.

Thus, in adopting a low main synchronous speed care needs to be exercised to ensure that serious vibration, having two or more nodes, shall not occur. Airscrews as a rule are very stiff in the plane of rotation and since various airscrews may be fitted to the same engine it is permissible to examine engines on the basis of a rigid airscrew and consider airscrew vibration as a separate matter when it arises. Owing to centrifugal effects the natural frequency of airscrew blades is a function of speed—a complication which does not arise in the ordinary consideration of torsional resonance.

I do not think gas pressure waves can have a damping effect as Mr. Bramson suggests. Some damping may be attributed to play in bearings, and in extreme cases the shaft restraints may increase under resonance conditions at the ends of the swing with resulting tendency to diaphragm and bearing cap failure.

At normal running speeds, the frequencies of engine disturbances are much higher, I believe, than natural frequencies in the structure of the machine. Mr. Douglas has not accused engines of causing any of the machine vibrations he has investigated. Although torsional vibration in a single throw engine may not produce very marked outside effects, the same cannot be said of multi-crank engines, which on the test bed, at any rate, give rise to most unpleasant vibration in the vicinity. In the case of the Arab engine, a vibration which felt like a continuous electric shock was transmitted to the cockpit.

Mr. Oswald spoke about reaction on engine mountings. As far as the machine is concerned it is the polar moment of the engine, including the pistons, which is concerned in torsional vibration on the mounting.

Taking the case of the radial engine with which the experiments described were made, it would require a mounting stiffness of about 70 tons ft. per degree twist to make so large a mass resonate with the engine impulses at 1800 r.p.m.; furthermore, the angular oscillation of this mass, at the same speed, under the varying reaction of the cylinder walls, would be only 0.0015 degree in amplitude if the mounting had no stiffness at all. From this it appears that as far as high frequency vibrations are concerned the aircraft engine mounting in a machine is insufficiently rigid to produce resonance effects. It is possible, however, that with such an engine attached to a very rigid bed, this rigidity may be adequate to give resonance with resulting trouble.

Mr. Fearn gives the equations for natural free vibration of a damped system. He points out that the frequency is affected by damping and that the phase change due to damping is small except in the region of critical speeds, as is indicated in Fig. 9c.

The exponential term $Ae^{-\lambda t g/2I}$ gets progressively less with time, and in dealing with forced vibrations it has been assumed as is usual that natural vibrations have been damped out. Forced vibrations occur at the same frequency as the forcing influence, and in working out the equations one is not troubled by the changes in frequency brought about by damping.

Mr. Fearn will see how this works out by reference to R. and M. 1053, in course of issue, in which expressions for phase change are given.

The mean kinetic energy of the piston is only equivalent to half the piston mass at the crankpin when the obliquity is neglected, as Mr. Fearn points out. Where there are two cylinders at right angles with pistons acting on the same crankpin, the kinetic energy of one piston is proportional to $\sin^2\theta$ when that of the other is proportional to $\cos^2\theta$ and the total kinetic energy, neglecting obliquity, is thus constant. Similarly, for a radial engine with slipper big-end the total kinetic energy is represented by the following series:—

$$MR^2/2g\,[\dot{\theta}^2\Sigma\sin^2\theta + 2\dot{\theta}\Sigma(\dot{\phi}\sin^2\theta) + \Sigma(\dot{\phi}^2\sin^2\theta)],$$

the summation being made for $n$ cylinders and, neglecting obliquity, the equivalent flywheel has a constant value, $MR/2g \cdot n/2$, which represent half the total piston mass at crank radius.

In a six-cylinder line engine, the total kinetic energy of all the pistons would be constant were it not for obliquity effects. There is thus a variation of the total inertia of the system* and corresponding to this variation of resultant inertia torque occurring at three cycles per shaft revolution, *i.e.*, of the same frequency as the engine impulses for a four-stroke engine. This explains the observed fact that a six-cylinder line engine may vibrate more violently when being motored than when running under its own power in the main synchronous speed region, the forcing harmonic being practically a pure sign curve when it results from inertia only. Vibrations at sub-multiples of the main synchronous speed are not observed when motoring because the forcing torque has then no appreciable harmonics.

I regret to say that at present I have no concise information to give Mr. Fearn for calculating crankshaft stiffnesses.

The results of some stiffness tests have been published† and an analytical method is given by Timoshenko.

There is still scope for work to be done on mathematical investigations and stiffness tests in order to obtain formulæ which can be used with confidence. This much guidance it is possible to give: In the case of a short throw shaft of robust design for the marine engine already mentioned, the stiffness between adjacent

* The effects of varying inertia have been studied by Dr. G. P. Goldsborough, see *Proc. Roy. Soc.*, Series A, Volume 109, page 99, and Volume 115, No. A.764.

† See article by H. Carrington, *The Engineer*, May 5th, 1926.

crankpin centres was about one-third that of one journal. Stiffness test on the " Sunbeam Arab " engine revealed that the corresponding ratio was about one-fourth. Tests with an aircraft engine shaft having long throws and rather thin webs have shown this ratio to be less than $\frac{1}{8}$th. It should be noted that in general, to a first approximation, one-third of the length of a shaft comprises journals, one-third crankpins, and the remaining third webs, and that the tendency in aircraft engines is for the shaft to be less stiff than a continuous journal.

Wing-Commander Cave is rightly anxious that torsional resonance shall not be associated specially with compression ignition engines. I would leave out reference to such engines had not some parts of the discussion centred round my remarks thereon, for I should not like to impede, in however small degree, a development which I believe merits all the assistance it can get. The remarks in question were prepared when I hoped to justify the broad title of the lecture, and they have now been modified to prevent misunderstanding. Wing-Commander Cave asks me to describe how best to deduce the effect of a solid friction type of damper. This is a matter I have touched upon in reply to Major Low's contribution to the discussion, which contribution goes to show that the same theory may be applied to either type in a general way. It may not seem profound to remark that a solid friction damper is either working or it is not; and yet this gives the key to investigating its behaviour. When the torque in the drive does not reach a value equal to the friction torque setting the damper is only in effect an added flywheel, and, if we regard it as such in calculations giving torque in drive, the conditions under which slipping will begin and cease can be inferred together with the effects on vibration when slipping is not occurring. The slipping torque for the damper is given an upper value by the strength of drive available. For the range of conditions under which slipping is estimated to occur, the effects may be determined approximately by imagining the damper to be replaced by a viscous friction damper of the same inertia moment and with a setting that makes the maximum torque developed in it equal to $4/\pi$ times the slipping torque of the constant friction type.

The horrible term to which Commander Cave takes exception refers as stated to the movement about the axis of symmetry of the balance weight, which axis rotates and is always normal to the crankshaft axis.

As regards changes of design to alter the position of a synchronous speed, the fact that such changes may need to be large will be realised by considering that to double a frequency, stiffnesses must be increased four-fold if the masses are unaffected, but it is probable that masses would be increased, in which case the increased stiffness necessary would be still greater. Similar considerations apply to the matter of lowering synchronous speeds. All this goes to emphasise the point Commander Cave makes that the important requirement is to be able to calculate the periods, etc., in the design stage and adjust the design accordingly.

I think the crankcase can be considered rigid as far as its effect upon shaft resonance is concerned, but this is a point upon which further information from tests is needed, as is also the final point raised by Commander Cave, respecting the width of resonance peaks.

In the absence of experience to the contrary one can only adopt the peak widths given by the theoretical resonance curves, which err, if at all, on the safe side. In this connection one must not assume that vibration is " cured " when the more violent manifestations have been removed. The amount of resonance effect necessary to produce abnormal gear noises must vary with the engine, and the incidence of these noises may not coincide with the incidence of excessive torque variation in the shaft. The amount a shaft has to wind and unwind to accommodate itself to engine torque variations alone is small, and this might quite conceivably be doubled or trebled by resonance effects before torque reversal

occurs at either main gears or auxiliary drives and makes manifest the fact that the twist variations are being amplified.

With Mr. Fedden's observations I am in full agreement. He confirms as far as single-crank engines are concerned the importance of considering torsional vibration in the early stages of design and mentions certain matters which call for further investigation to make calculations from drawings more accurate. Readers concerned with multi-crank engines should find helpful a paper read before the American Society of Naval Architects and Marine Engineers in November, 1925, by F. M. Lewis. This paper is entitled "Torsional Vibration in the Diesel Engine." It describes practical methods of making calculations, and contains a good Bibliography.

# INFORMAL DISCUSSIONS

## OIL COOLING

On Tuesday, January 18th, 1927, an Informal Discussion was held in the Library of the Society on "Oil Cooling," Colonel the Master of Sempill, Chairman of the Society, in the chair.

Mr. A. H. R. FEDDEN, opening the discussion, said:—

### *The Necessity for Oil Cooling*

The necessity for oil cooling in aero engines arises from the fact that the lubricating oil circulates through the engine for hours on end, and, in passing through, the temperature is raised owing to bearing friction and conductivity from hot parts of the engine, the latter factor becoming the more important on modern high efficiency engines, closely cowled to reduce drag.

Although judicious arrangement of the oil circulation system external to the engine will ensure a certain amount of cooling, the possibilities are limited and temperature control is impracticable. The total amount of oil in circulation has little effect on the final running temperature, though it does affect the rate of temperature increase. Short tests are therefore of little value, and for long distance work it is the total cooling area available which controls the final running temperature.

The air speed is also a vital factor, from theoretical considerations under similar conditions, to obtain the same heat dissipation at 75 m.p.h. as at 100 m.p.h. it would be necessary to approximately double the cooling area.

### *Difficulties of Cooling Oil*

Oil is a particularly difficult medium to cool, owing to its relatively high viscosity, with consequent difficulty in obtaining a turbulent flow, and its low heat conductivity, which at low temperatures allows the formation against the cooler walls of an insulating layer of thick oil, considerably reducing the efficiency of the cooler, and in some cases blocking the passages completely.

### *Relative Problems on Water and Air-Cooled Engines*

With the increased bearing loads on modern engines there has been a tendency to considerably increase the flow of oil through the engine in an endeavour to get the bearings to stand up. In the case of water-cooled engines of the in-line type, with a number of heavily loaded bearings, the flow being increased in some cases up to 300 or more gallons per hour, with a considerable increase in oil cooling difficulties.

With the air-cooled radial engine, especially of the single bank type, having one main bearing, the oil flow can be considerably reduced and the cooling difficulties proportionately decreased, even though the actual temperature rise may be increased, due to the comparatively small area of crankcase available for heat dissipation.

On the Bristol Jupiter VI. with a floating bush big end and hardened sleeve, a flow of approximately 40 to 50 gallons per hour is quite satisfactory, even with P.V. factors of 17,000 to 20,000.

### *Installation of Oil Coolers*

The most suitable position is on the pressure side of scavenge pump, as slight leaks are less serious here and more readily detected.

The maximum air flow to the cooler should be available under least favourable conditions, usually on the climb when the slipstream provides the maximum effect.

Oil thermometers are essential and should preferably be of the mercury invar type, particularly for altitude work. They should be installed as close to the engine as possible, one in the feed system and one in the scavenge system. To ensure true readings the thermometer must be in the direct flow of the hot oil, and also should not be housed directly in a hot part of the engine.

The permissible temperatures vary with the oil, and to a certain extent with the engine; on the Jupiter, with Castrol R oil, a feed temperature of approximately 50° is desirable, with 70°C. as a maximum, and a maximum permissible return temperature of 100-105°C.

### *Types of Oil Coolers*

Besides cooling efficiency, which is essential, weight, drag, cost and service efficiency must also be considered. Standardisation on a few types to suit the majority of machines is desirable and is possible on a very small range for any particular engine without sacrifice of quality, apart from drag, a consideration of which would entail a special cooler for each type, with a comparatively slight gain, except on really high speed machines. Standardised coolers of known performance can be advised in advance from a consideration of machine performance and operating conditions, giving a saving in first cost and in experimental delays on the actual machine.

For the Jupiter engine three coolers have been standardised, all of the cylindrical type, the cooling air passing through tubes running longitudinally through the cooler. Suitable baffles ensure the diffusion of the oil and a bye-pass valve safeguards the cooler against excess pressures when starting up with cold oil, and short-circuits the cooler if the oil is already at too low a temperature.

These coolers weigh from 3.25 to 8.5 lbs. and give a comparative drop across the cooler of from 26°C. to 45°C., according to size. These temperatures are comparative only for one set of conditions, as unfortunately no approved formula exists for predicting the results with varying atmospheric and oil inlet temperatures.

Many other types of coolers exist, but usually suffer from drawbacks, chiefly from the efficiency and reliability in service point of view. In some cases attempts have been made to fin the oil tank and utilise it as a cooler; this can be quite satisfactory on a fast machine, but usually limits the designer in his location of the tank and does not permit any simple form of control.

For the material of the cooler, metal of good heat conductivity is desirable, but the permissible thickness of metal should also be considered, making it sometimes advisable to sacrifice heat conductivity for greater strength.

All coolers should be pressure tested before use, owing to the high pressures generated when starting with cold oil. The pressure requirements vary with types of engines, being usually greatest on engines with high oil circulation; in the case of the Jupiter coolers a pressure test of 50 lbs. sq. inch is quite satisfactory, owing to the comparatively low oil circulation and the inclusion of a reliable bye-pass valve.

There followed an interesting discussion. One speaker asked whether any experiments had been carried out with oils of a lower viscosity than normal, as it was possible one would gain more than one lost in efficiency. Another pointed

out that the oil temperature was much higher on cowled than uncowled engines, and suggested that engine designers should consider more carefully the design of engines from an aerodynamical point of view. Mr. Fedden was asked if any attempt had been made to cool the oil by means of a leading edge radiator. A number of speakers raised the point that some device should be installed to indicate to the pilot when the oil temperature was getting too great.

Mr. FEARN pointed out that in a recent lecture given before the Society, Sir Alan Cobham related how an attempt was made to improve the cooling of the oil by the addition of an extra radiator. The combination of the two radiators, however, gave a still smaller drop in temperature from the inlet to the outlet.

This haphazard method of dealing with cooling problems suggests that improvements would ensue if our designers were to return to the year 1874 and consult Prof. Osborne Reynolds' work on the subject.

Concisely, from the principles then expounded, we may deduce the following equation for the simple case of a single tube placed in a transverse current of air :

$$\Delta\theta = A\left[1 - (1/e^{Rl})\right]$$

where $\Delta\theta$ = temperature drop from the inlet to the outlet of the tube.
$l$ = length of the tube.
$A$ and $R$ = constants, depending on the cross-sectional area, the inner and outer peripheries, material, etc., of the tube.
$e$ = base of the Napierian Logarithms.

From this we see that the temperature drop is an exponential and not a linear function of the length of the tube, and should suggest to our designers an ideal, *i.e.*, the long single tube cooler.

Mr. FEDDEN, in the course of his reply, pointed out that oil coolers were not as efficient with pure oils as with blended oils. He agreed with one speaker that the elemental type of cooler was the best, but suggested that coolers should be specially designed for each machine to get the best results. Leading edge oil coolers had been tried and were very successful. He thought it was almost impossible to standardise oil coolers, however. One interesting oil cooler which had been produced had an impeller which blew the oil in a thin film over a large area.

## COMFORT IN AIR TRAVEL

On February 7th, 1927, a dinner was given by the Royal Aeronautical Society in rooms kindly placed at the Society's disposal by the Royal Aero Club. It was attended by as many Fellows, Associate Fellows and others for which there was accommodation, and was followed afterwards by an Informal Discussion on "Comfort in Air Travel."

Introducing the opener of the discussion, Air-Commodore J. G. Weir, C.M.G., C.B.E., F.R.Ae.S., Colonel THE MASTER OF SEMPILL, A.F.C., A.F.R.Ae.S., Chairman of the Royal Aeronautical Society, who presided, said that the recent historic flight of Air-Commodore Weir to Cairo in the "Hercules" air liner had occurred to him as giving a suitable opportunity for discussion on the important subject of air comfort.

Air-Commodore J. G. Weir's opening covered the ground very fully and ably and was afterwards discussed by the many speakers. The guest of honour was Air Vice-Marshal Sir John Higgins. Among the speakers were Lieutenant-Colonel W. A. Bristow, Major Mayo, Mr. Handley Page, Professor A. M. Low, Mr. J. D. North, Mr. Chorlton, Captain Leverton, Air Vice-Marshal Sir Vyell Vyvyan, Major Brackley, Mr. Grey, Captain Tymms and Colonel Mervyn O'Gorman.

The following is a brief summary of the tenor of the discussion :—

The dominant basis of comfort in the air was stated to be a sense of security. Comfort would be substantially achieved if one felt convinced of the absence of the unintentional in the proceedings—this psychological impression would be assisted by size—room for one's knees, room to move about. The distress due to bumps and irregular movements which conduce to air sickness would be diminished both by large size and by heavy wing loading—though both have very important limitations. Beyond this limit, the first entailed grave uneconomical structure, the second fast landing. The latter led to increase in the size of landing grounds. Thus there comes the question of increased cost immediately to obtain increased comfort. Something could be done to avoid exaggerated expense on landing grounds and yet ensure the sense of security by utilising to the full the knowledge now existing for obtaining control of flying below the stalling speed.

Large size had also the merit of being compatible with the increased number of engine units desirable for reliability. It was not demonstrated that the existing large engines used in threes were in fact the most economical size of engine unit. Thus the carrying of a 33 per cent. margin of engine power as a security against forced landing was a money question of importance. Perhaps with four engines the same security could be got at the less expense of a 25 per cent. margin. This has yet to be proved out, but it was well to learn that Imperial Airways was prepared to face the 33 per cent. in carrying out its policy of safety first.

Noise was one of the discomforts of air travel at present, not only in itself but because of its fluctuation and pulses as produced in multiple-engined craft. It brought the attention back to the unusual situation of the air traveller. If his mind could be suitably diverted, even though all noise could not be eliminated, it seemed possible, judging by common experience of sleeping on railways, that some classes of uniform sound (to which aircraft noises might be reduced if only it were known what to seek for) can be forgotten or overlooked.

It was thought that airscrew noises were more serious than those from the exhaust, and that engine noises, tappet and gear noises, etc., could be got rid of only by grave expense in the form of sacrificed efficiency. This meant using larger engines as well as involving an increased outlay in the preparation of silent parts or gears.

As regards airscrew noises, it was a question for experiment whether top speed was the dominant cause, since the speed of revolution could be lowered by using geared engines. It was *not* remarked that high-pitched and low-pitched airscrew noises were possibly due to different causes, and would probably require an entirely different mode of attack, but it was suggested that low notes caused less discomfort than high. This has not, as yet, been proved by experiment. It was asserted by some that slower airscrew speed would both lower the notes and diminish the noise. One speaker stated that in an aeroplane with no external struts or stays he found the noises unobjectionable. Much might be done by so supporting the engines as to damp out each vibration which occurred, so that it did not repeat itself or get transmitted to the travellers. The sound insulating of the cabin was advocated. Many people were capable of suffering air sickness from the pulsation of the noises alone. Seclusion for the air sick was desirable, and this would also allow each individual to have command of his own ventilation without incommoding others. Air sickness did not occur in more than five per cent. of travellers. It was caused not only by atmospheric bumps, but by unsteady pilot work.

The flyers on an air liner did, by wireless communication to each other, indicate how bumpy places could be circumnavigated by a following aeroplane. A more general way of avoiding bumps and the cause of air sickness was to

utilise the reliability afforded by three engines, and to abandon piloting by sight of the ground by flying at such heights—say, 10,000 or 12,000 feet—above the clouds and in straight air. To be above the clouds meant navigating instead of only piloting, and for this abandonment of the sight of the ground, and with it the possibility of looking out continually for possible landing places, it was necessary to have at least the three-engine standard of reliability with 33 per cent. margin of power.

The design of comfortable light chairs was discussed and found to be difficult of attainment because of the non-existence of a standard man. Spaciousness was necessary.

These discomforts, minor as they might appear to be, were, when added together, significant. One speaker declared that they meant that five hours a day flying was as much as many people would stand, and consequently he was for the present willing to consider the airship more suitable for any longer continuous flights.

A suggestion was made that those whose opinion on flying risks was measured in money, the Insurance Companies, could greatly add or indeed establish the sense of safety in the air by charging nominal premiums of, say, one shilling for the London-Paris flight in chosen aircraft. They would no doubt require some form of Government guarantee, but any big reduction of insurance charges would have a valuable and encouraging effect, both psychological and financial.

Mr. W. D. SAVAGE (*communicated*): Owing to the lateness of the hour at which the discussion was thrown open I refrained from taking part in it, but I should like to make the following remarks:—

At the International Air Congress held in London in 1923 I had the honour of joining Mr. Handley Page in presenting a paper to the Air Transport Section of the Congress, entitled "The Development of Commercial Aviation."

On page 694 of the Report of the Congress the following paragraph appears:

> "One of the most important considerations from the point of view of serious improvement (of the aeroplane) is that of the comfort of the passengers. The aim to-day appears to be to crowd as many souls as possible into the smallest capacity possible, and cabins are generally uncomfortable. They are either draughty or badly ventilated; and, if at all, very badly heated. Sanitary provision in case of air sickness should be incorporated in the cabin equipment, which, together with good ventilation, would prevent to a very great extent air sickness occurring at all."

A little further on we read:

> "Some consideration, too, might be given to the use of the pusher type of engine installation, to lessen the noise in the cabin from the propeller. The old type of pusher machine used during the war was more silent than the tractor from the point of view of the occupants of the machine, the great advantage being that all the noise is left behind. In a similar way, if it were possible to apply pushers for commercial work, propeller, engine valve and other noises would not be heard so readily in the cabin."

Four years later almost identical suggestions are put forward with the same end in view, the comfort of the air traveller, but judging by the remarks which have been made in this discussion, no improvement has taken place.

I entirely agree with Major Mayo that the high "revving" airscrew has not improved matters; in fact, I think it has made things worse. I think the question of air sickness has been overstressed and would like to iterate my opinion, as already quoted above, that given reasonable sanitary accommodation and good ventilation there would be very little air sickness.

On page 698 of the aforementioned Report the following paragraph occurs:

> "Some method or material for fixing engines which will insulate the machine and cabin from the engine vibration would be welcome. Fibre has been tried, as insulating blocks, but no appreciable improvement being observed and atmospheric conditions violently affecting its dimensions and elasticity, it was abandoned."

I feel, with Captain De Havilland, that instead of impairing efficiency of the engine, the question of insulation and damping should receive more attention.

In conclusion, I would say that the speed of 100 miles per hour has become the generally accepted economic cruising speed for passenger aircraft, but, I submit, without any facts or figures to substantiate it other than that of being able to maintain a higher ground speed in a 40 m.p.h. wind than that of surface travel.

It appears to me that everything is sacrificed for performance and paying load, and I suggest that it would pay, in the long run, to reduce the cruising speed and paying load ten per cent. and use the thus available power to carry the extra weight of a few improvements for the general bodily and mental comfort of the passengers, for which, after all, the machine is designed and subsidised.

## MATERIALS, WITH SPECIAL REFERENCE TO CORROSION

On February 15th an Informal Discussion on "Materials with Special Reference to Corrosion" was held in the Library of the Society, Captain R. O. King, of the Air Ministry, in the chair.

Mr. W. M. Dyson, who opened the discussion, said: Before opening this discussion I have to say that any opinions I state or suggestions I may make during the discussion are my own and not necessarily those of the Air Ministry.

I believe the subject for discussion is quoted in the Journal of your Society as "Aircraft Materials, with special reference to Corrosion." I propose to confine my remarks almost entirely to aircraft metals and corrosion, and shall deal with the subject in general.

In my opinion the problem of metal corrosion on aircraft has been somewhat neglected by the aircraft manufacturer, but the rapid development of all-metal machines, the ever-increasing use of lighter sections and the employment of different metals in contact with each other tend towards increasing the problem. The seriousness will be at once apparent to those of you who have an opportunity of examining a land machine for service with the Fleet Air Arm. A few weeks' exposure on the deck of an aircraft carrier makes a sad mess of all external metal parts of a machine.

I suggest that corrosion has to be considered, not only from a maintenance standpoint, but also from that of safety; if we must have members made from very thin high tensile strip, they should be adequately protected; short continuous corrosion of 26G steel might be very troublesome on particular members.

I do not propose to deal with the vexed question of the chemistry of corrosion; valuable fundamental work is proceeding at various institutions, and doubtless ultimately will be of value in assisting the prevention of corrosion. I am going to deal with present methods of protection, their usefulness or otherwise, and shall then discuss briefly alternatives which have proved satisfactory on experimental machines.

Taking steel first, not because it is considered a more important structural material than the lighter metals, but because we have had more experience with steel.

The standard method of protection, as most of you are aware, is by painting, externally and internally; by painting I include all forms of organic protectives, enamels, lacquers, etc., howsoever applied. Reliable organic coatings are satisfactory and give a reasonable life internally if kept dry; exposure to moisture generally breaks down the coating, frequently corrosion occurs with pigmented coatings unobserved under the film of wet paint.

Externally, paints might be classed unsatisfactory even on a straight land machine, and almost useless on seaplanes and flying boats unless you are prepared to shoulder prohibitive maintenance and inspection charges.

Treatment of steel with metal coatings—or one of the oxidising processes—and subsequent painting is adopted by one or two manufacturers, providing zinc or cadmium and not tin or nickel be the case, considerable improvement over paint alone is obtained. It is suggested, however, that the effect of painting on metal coating may be useful for decoration, but may be a disadvantage where the paint is liable to become saturated with any corroding medium, water alone being a medium.

I cannot understand why the aircraft industry has not more widely adopted zinc coating as a protective; why paint is preferred for metal on aircraft and galvanising for metal roofs of aircraft factories is confusing to me. It is a fact that a sound coating of zinc or cadmium, no thicker than half a thousandth of an inch, gives a considerably longer life than the most satisfactory paint under any conditions met with on aircraft. The metal coating may be secured by electro-deposition, or, where applicable, by one of the hot processes.

No claim is made that zinc or cadmium coated steel is immune from corrosion under particular conditions; instances can be quoted where it is essential that a non-corroding metal be used if a reasonable life is desired.

Steels are now available entirely resistant to corrosion under aircraft conditions, but at present they are a luxury; for many reasons these super-stainless steels are not likely to be considerably used on aircraft; they have, however, their special sphere and should not be ignored.

Increased use and experience with what I shall call ordinary non-corrosive steel is definitely suggesting that we have in this material a possible solution of most of our corrosion problems. It is not suggested nor considered necessary that all steel on aircraft should be of the non-corrosive variety, but with the existing alternatives I do consider it would be advantageous if this steel could be more generally adopted.

At present it is not possible to substitute a non-corrosive for all the numerous ordinary steels employed on aircraft without some sacrifice; the majority can, however, be adequately replaced, and it is safe to predict that in a short time there will be available non-corrosive steels suitable for all aircraft parts; cables and streamline wires are now under investigation.

I know that several manufacturers are averse from the use of non-corrosive steels, but I feel sure they will in time overcome their aversion and fall into line with other branches of engineering, with advantage to the users of aircraft.

So much for steel. The problem with light metals and alloys is by no means so bright as with steel. Compared with ordinary steel, the best of the light alloys, under all conditions, corrodes out of all proportion to the respective specific gravities. What is worse, the corrosion may be intercrystalline and not revealed until the object reaches an unsafe condition. Numerous examples have been observed under service conditions, where the metal has become "rotten," although the film of paint was apparently intact. Paint cannot for long protect parts or places liable to saturation with moisture; in this respect, floats and hulls are particularly susceptible. Contact with other metals should be carefully avoided. In this connection, there was a very good example on a flying boat reconditioned

a short time ago. On one side of an aluminium alloy hull the wing root fittings were made of ordinary steel; on the other side, ordinary non-corrosive steel was used; aluminium and steel were painted before assembling. It was revealed that the light metal in the vicinity of the ordinary steel was corroded to the state of collapse, likened to a sponge; in the vicinity of the non-corrosive steel, corrosion was almost negligible.

Protection of light metals might be considered similarly to steel, always remembering the different rate of corrosion once the protective breaks down; paints offer least protection, metal protectives are applicable just as with steel, but successful application is more difficult.

The alternative non-corrosive light metal does not exist commercially, as with steel; at present, therefore, there appears to be only the anodic process which can be considered useful. I willingly admit this process has limitations, but so long as light metals and alloys remain with us in the present circumstances, the anodic film is undoubtedly the only reliable protective. I feel sure, had the process not been available, the development of light metals could not have reached the present stage. It is suggested that, in all light alloy aircraft work, first consideration should be given to the corrosion problem.

My remarks are intended to apply to aircraft, excluding engines; fortunately, this unit, apart from the water system of the water-cooled type, has given little corrosion trouble.

Captain KING pointed out that the subject was of great economic importance. The harmful effect of corrosion was of the utmost concern not only to aircraft designers, but to all who had to deal in metals. In the common case of the corrosion of steel it would be quite possible, if it were not for the preventive measures taken, that the annual loss would equal the annual production. In the aircraft industry the prevention of corrosion was of greater relative importance than in any other because we cannot afford to counteract the evil by increasing the original weight. Corrosion, too, frequently occurred in such a way that it could not be dealt with effectively by mere increase of weight. The aircraft industry depends on light alloys whose properties are not completely understood.

Mr. MANNING pointed out that stainless steel had given a considerable amount of trouble in the past, especially soft sheet. Now, however, stainless steel was giving satisfactory results. In the case of light alloys, though no corrosion appeared on a specimen under test, corrosion appeared when the alloy was used in a machine. Moreover, the method of preventing corrosion, say, in the North Sea was all right, but not in harbours, where chemicals may cause corrosion. He was not sure the anodic process was any good, especially with regard to floats.

Wing Commander T. R. CAVE-BROWNE-CAVE pointed out that the reliability of the paint or material used to prevent corrosion was important, as against other methods which might lead to mechanical defects in the metal itself.

Mr. HARDY suggested that a metal coating was best against corrosion, though there are some advantages of an organic coating. A good organic coating should give protection for some eighteen months to two years.

Mr. DOUGLAS pointed out that a structure may be heavily corroded and yet still stand up to its work. Methods of corrosion should be applied to the more vulnerable points. In tubular struts the most vulnerable points are the centre and end fittings. These are the points where protection is needed. The free lips of built-up steel spars should be specially protected. Would it be possible to have a protective paint which would change colour when corrosion took place, and so indicate the danger?

Mr. DYSON, in replying, stated that the anodic process seemed to hold out the greatest possibilities. It had been tried on the floats for the Schneider Cup entries and had proved satisfactory. Paints which change colour on corrosion have been tried to some extent.

# THE FLAPPING FLIGHT OF BIRDS. II.

BY PROFESSOR SIR GILBERT T. WALKER, C.S.I., SC.D., F.R.S.

## *Introduction*

In a paper in the number of this journal for November, 1925, it was shown that if a flying machine were fitted with wings of standard section and these were flapped in a rhythmical manner, the machine would be supported and propelled, its weight, dimensions and velocity being those of a typical bird; the degree of accuracy attempted in the analysis did not exceed five per cent.

A subsequent examination of the power involved established a high efficiency; but this was due to a large amount of negative work during up-beats; and although such conditions might be maintained in a mechanical model they were unlikely in a bird, which has a large muscle, the pectoralis minor, for lifting its wings. In a bird efficiency seemed to require that the angle of incidence of the outer portion during an up-beat should be negative (see §7, p. 593, of the previous paper); the angle was however made zero (see §9) in order to secure adequate lift, and it is this feature which gives rise to the negative work. Study of some very interesting "slow motion" cinema films of British Instructional Films, Ltd., showed that in some birds, at least, the up-beat occupies only two-thirds of the time of the down-beat; and the introduction of this characteristic, which is indicated on p. 7 of the first report of the Bird Construction Committee, leads to a marked improvement in the results of the analysis. We accordingly discuss first the amount of support and propulsion provided under the new conditions and then the amount of work involved.

2. We shall use the notation of the previous paper except that suffixes 1 and 2 will distinguish the down-beat and the up-beat. Thus the distance $a_1$ described during a down-beat will be related with $T_1$ the time of a down-beat by $VT_1=a_1$, where $V$ is the forward velocity of the bird; similarly $VT_2=a_2$. And if $n$ is the number of complete flaps in a second $n(a_1+a_2)=V$, or $n(T_1+T_2)=1$; so $T_1/3=T_2/2=1/5n$. Further, as the ratio of the flapping velocity of the wing tip to the forward velocity we have $k_1=2la/a_1$, and $k_2=2la/a_2$. So as we shall suppose that the time of a down-beat is the same as in the previous paper $T_1=T$, $a_1=a$; and $k_1=.42$, while $k_2=.63$. During the down-beat we assume that $\beta$, the angle of incidence, is 12° all along the wing, and the section is "high lift" or R.A.F. 19; during the up-beat the inner third has the same conditions as in the down-beat, but the middle third has $\gamma=4°$ and "high velocity" section R.A.F. 15; and the outer third has $\gamma=-10°$, the section being flat.

3. From §2 of the previous paper we see that the total momentum in the forward direction, added during a down-beat, per unit area at a point $r$ in the wing will be $\rho V'_1(2L_1ra-D_1a_1)$; and the average force of this kind during a number of complete beats, each lasting $T_1+T_2$, will be obtained by dividing by $(T_1+T_2)$ or $5a_1/3V$; thus it is $.6\rho VV'_1(2L_1ra/a_1-D_1)$, or $.6\rho VV'_1(L_1k_1r/l-D_1)$. Now $V'_1=(1+k_1^2r^2/l^2)^{1/2}V=(1+.09\,r^2/l^2)\,V$, omitting terms less than 1/100 of that kept; so the time average propelling force from the down-beats is

$$.6\rho V^2(1+.09\,r^2/l^2)(.42\,L_1r/l-D_1).$$

In a similar manner from §2 we find an upward momentum added during a down-beat $\rho V'(L_1a_1+2D_1ra)\sin a/a$, and the average upward force arising from the down-beats will be $.54\,\rho V^2(1+.09\,r^2/l^2)(L_1+.42\,D_1r/l)$.

On integrating these forces over the outer thirds of the wings* we find propelling and lifting forces, the total area of the wings being $S$, $S\rho V^2$ $(.074\,L_1 - .212\,D_1)$ and $S\rho V^2$ $(.191\,L_1 + .066\,D_1)$. From the middle thirds we find $S\rho V^2$ $(.043\,L_1 - .205\,D_1)$ and $S\rho V^2$ $(.184\,L_1 + .039\,D_1)$, and from the inner thirds, $S\rho V^2$ $(.014\,L_1 - .201\,D_1)$ and $S\rho V^2$ $(.181\,L_1 + .013\,D_1)$.

4. Turning now to the up-beats the total forward momentum added (see §3) being $\rho V'_2\,(-2\,L_2 r\alpha - D_2 a_2)$, the average forward force during a complete beat lasting $T_1 + T_2$ or $5a_2/2V$, will be

$$.4\rho V^2\,(1 + .20\,r^2/l^2 - .02\,r^4/l^4)\,(-.63\,L_2 r/l - D_2)$$

and the average upward force will be

$$.36\rho V^2\,(1 + .20\,r^2/l^2 - .02\,r^4/l^4)\,(L_2 - .63\,D_2 r/l).$$

On integration we find, omitting the factor $S\rho V^2$ in each case, propelling and lifting forces on the outer thirds,

$$(-.079\,L_2 - .151\,D_2) \text{ and } (.136\,L_2 - .072\,D_2);$$

on the middle thirds,

$$(-.044\,L_2 - .140\,D_2) \text{ and } (.126\,L_2 - .040\,D_2);$$

and on the inner thirds,

$$(-.014\,L_2 - .134\,D_2) \text{ and } (.121\,L_2 - .013\,D_2).$$

5. Introducing now the appropriate values of $L$ and $D$ we have during a down-beat $L_1 = .797$ and $D_1 = .0865$ for each third of the wings. The propelling and lifting forces on the whole wing during down-beats contribute to the time averages,

$$(.131\,L_1 - .623\,D_1) \text{ and } (.560\,L_1 + .119\,D_1),$$
$$\text{or } .051 \text{ and } .453 \quad . \quad . \quad . \quad . \quad . \quad \text{(A)}$$

Towards this the contributions from the outer, middle and inner thirds are .040 and .158, .017 and .150, −.006 and .145.

During an up-beat the values of $L_2$ and $D_2$ for the outer, middle and inner thirds are −.340 and .067, .242 and .0148, .797 and .0865. Thus the propelling and lifting forces due to the outer thirds average

$$+.017 \text{ and } -.051;$$

from the middle thirds

$$-.013 \text{ and } +.030;$$

and from the inner thirds

$$-.023 \text{ and } +.095.$$

From the whole wing the propelling and lifting forces during up-beats average

$$-.019 \text{ and } +.074 \quad . \quad . \quad . \quad . \quad . \quad \text{(B)}$$

Combining with (A) we get from up and down-beats

$$+.032 \text{ and } +.527 \quad . \quad . \quad . \quad . \quad . \quad \text{(C)}$$

6. As we saw in §8 of the previous paper the weight of the bird is $.52\,S\rho V^2$, so that the lift is adequate. The propelling force has to overcome the resistance of the body, about $.0015\,S\rho V^2$, so that $.030\,S\rho V^2$ is available beyond this; now the ratio of .030 to .52 is $\tan 3\frac{1}{3}°$, and hence the surplus propelling force will enable the bird to climb up an incline of $3\frac{1}{3}°$.

7. If instead of $\gamma = -10°$ for the outer third we took $\gamma = -5°$, the propulsion would diminish to .025 and the lift increase to .55; on the other hand increasing $\gamma$ to $-15°$ diminishes the propulsion to .029 and the lift to .52. It would appear that the best value for $\gamma$ would be $-11°$ or $-12°$.

8. If we change $\beta$ from 12° to 14° during the down-beats we obtain instead of C

$$+.029 \text{ and } +.556.$$

* It is easily seen that $r/l$ integrates to $5S/18$, $S/6$ and $S/18$ over the outer, middle and inner thirds; $r^2/l^2$ integrates to $19S/81$, $7S/81$ and $S/81$; $r^3/l^3$ yields $65S/324$, $5S/108$, and $S/324$; $r^4/l^4$ yields $211S/1215$, $31S/1215$ and $S/1215$; and $r^5/l^5$ yields $665S/4374$, $7S/486$ and $S/4374$.

9. If we change $\gamma$ in the middle third during up-beats from 4° to 0° we obtain instead of C, +.041 and .504. This lift is inadequate, but by combining with the change just handled of $\beta$ to 14° we should get +.038 and .533, giving a climb of about 1 in 15.

10. If we go further and in §5 change $\gamma$ in the middle third to −4° in the up-beats, still with a high velocity section there, we should have $\beta=12°$ for the whole wing during the down-beats and $\gamma=-10°$, −4°, +12° in the up-beats. We should then increase the propulsion of §5 by (.044)(.087+.242) −(.140)(.0156−.0148), or .0144; and the lift by −(.126)(.329)−(.040)(.0008), or −.041. Hence we should have to combine the change with that of §8 and get final forces of .043 and .515.

As .515/.043 is 12 this would provide for climbing up a slope of 1 in 12 or nearly 5°.

11. It is of interest to get some idea of the effect of a pointed wing by considering a wing with the outer third narrowed in the ratio of 4 to 5, the middle third unchanged, and the inner third widened in the ratio 6 to 5. The consequence of the change would be to multiply the contributions from the outer portion by .8, and the inner by 1.2, those from the middle remaining unchanged; obviously the alteration in the total propelling and lifting forces is a fifth of the excess of the forces due to the inner third above those due to the outer third. For the down-beat the change in the propelling and lifting forces of §5 is −.009 and −.002, and for the up-beat −.008 and +.029; in all the changes in the forces are

$$-.017 \text{ and } +.027 \quad . \quad . \quad . \quad . \quad . \quad \text{(D)}$$

As we should expect, an increase in the area of the inner third, which provides most of the support, improves the lift, while a diminution in the outer third reduces the driving power. In order to improve the propulsion, which mainly comes from the first term in $\rho V'^2(-2L_2 r\alpha - D_2 a_2)$, $L_2$ being negative, we must either increase $V'_2$ or $r$ or $\alpha$. Thus the bird with more pointed wings than a rook must flap more often in a second (to increase $V'_2$), or have relatively longer wings (to increase $l$), or flap through a bigger angle $\alpha$. The effect on the result (D) of increasing $L_2$ by adopting a bigger angle of incidence will be prejudicial if this increases $D_2$ to a disproportionate extent; for example, a change of $\gamma$ from −10° to −15° will diminish the propulsion by .002 as well as the support by .009. Some improvement may however be gained by changing $\gamma$ in the middle third from +4° to −4°; the increase in propulsion is +.014 and the decrease in support .041. Thus a partly wedge-shaped wing with $\beta=12°$ throughout, and $\gamma=-10°$, −4°, +12° will have a propulsion of .032−.017+.014, or .029; and a support of .527+.027−.041, or .513, which is sufficient in view of the limits of error.

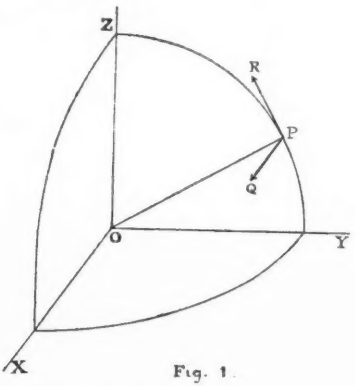



Fig. 1.

12. The work involved in flapping flight may now be considered. We saw in the previous paper (p. 590) that for a point $P$ on the axis $OP$ of the wing distant $r$ from the shoulder, the angle $YOP$ between $OP$ and the horizontal being $\theta$, the lift and drag per unit area were $\rho LV'^2$ and $\rho DV'^2$, and that the component of these along $PR$ was $\rho LVV' - \rho Dr\dot{\theta}V'$. Thus the rate at which work is done by the bird in increasing $\theta$ and causing $P$ to move at a rate $r\dot{\theta}$ along $PR$ is $\rho V' (-LV + Dr\dot{\theta})\, r\dot{\theta}$; the components of the lift and drag along $PQ$ will not affect the work done in flapping the wings.

During a down-beat the value of $\dot{\theta}$ is given by $l\dot{\theta} = -k_1 V$ and the limits of $\theta$ are $\pm\alpha$; so the work per unit area during an entire down-beat will be

$$\rho V'_1 (-L_1 V - D_1 V k_1 r/l)(-2r\alpha),$$

and as $T_1$, the time of a down-beat is $3/5n$ (§2 above), we have $2\alpha = -\dot{\theta}T_1 = (k_1 V/l)(3/5n) = 3k_1 V/5ln$. Hence the work per unit area during the $n$ down-beats that occur during a second will be $\rho V'_1 (L_1 V + D_1 k_1 V r/l)\, 3k_1 V r/5l$ or

$$\rho V^2 V'_1 (L_1 + D_1 k_1 r/l)\, 3k_1 r/5l \quad . \quad . \quad . \quad . \quad . \quad \text{(E)}$$

On using the value of $V'_1$ of §3 this becomes

$$.6\rho V^3 k_1 (1 + .09\, r^2/l^2)(L_1 + D_1 k_1 r/l)\, r/l$$

or

$$\rho V^3 \{ .25\, L_1 (r/l + .09\, r^3/l^3) + .106\, D_1 (r^2/l^2 + .09\, r^4/l^4) \}$$

Integrating over the outer third, using the footnote of §3 and omitting the factor $\rho SV^3$, the contribution towards the work becomes

$$.25\, L_1 (5/18 + .09 \times 65/324) + .106\, D_1 (19/81 + .09 \times 211/1215)$$
$$\text{or } .075\, L_1 + .027\, D_1.$$

Similarly the middle third yields $.043\, L_1 + .010\, D_1$ and the inner $.014\, L_1 + .001\, D_1$.

13. The work involved in an up-beat, for which $l\dot{\theta} = +k_2 V$, is derived from $\rho V^2 V'_2 (-L_2 + D_2 k_2 r/l)\, 2k_2 r/5l$, corresponding to (E); this amounts in a second to

$$.4\rho V^3 (1 + .2r^2/l^2)(-L_2 + .63\, D_2 r/l)(.63\, r/l).$$

Over the outer third, omitting the factor $\rho SV^3$, we find that we need a power of $-.080\, L_2 + .043\, D_2$; over the middle third, $-.044\, L_2 + .014\, D_2$; and over the inner third, $-.014\, L_2 + .002\, D_2$.

14. Let us apply our analysis to the wing of §2. If in the results of §§12, 13 we insert the values of $L_1$, $D_1$ appropriate for a high lift wing at 12° (*i.e.*, .797 and .0805) we find that power for the three thirds of amounts .062, .035 and .012 is needed during down-beats. During up-beats for a flat outer third inclined at −10° (with $L_2 = -.34$, $D_2 = .067$) we require power .030; for the middle third of high velocity section inclined +4° (with $L_2 = +.242$, $D_2 = .0148$) the power is −.010; and for the inner third, high lift at 12°, we find −.011. Thus the power involved in the down-beat is .109 and in the up-beats .009, or $.118\, \rho SV^3$ on the whole.

15. For the conditions of §10 we have to change $\beta$ from 12° to 14° during the down-beats, so that $L_1 = .845$, $D_1 = .101$, and the power requisite for these is .116. For the middle third with $\gamma = -4°$ instead of +4° we take $L_2 = -.087$ and $D_2 = .0156$, so that the power for this segment is +.004. On the whole wing the power needed for the up-beats is then .023, and for complete flaps .139 instead of .118.

The change from §5 to §10 increased the propelling force from .032 to .043, a change of 34 per cent., and is secured by an increase of only 18 per cent. in the power required.

16. Let us now consider the power required for the partly pointed wing discussed at the end of §11, with $\beta = 12°$, and $\gamma = -10°$, −4° and 12°. In order

to get the power for the down-beat we take the former value .109 and diminish it by one-fifth of .062 − .012; we obtain .099. For the up-beats the three portions need (.030 × .8), (.004) and −(.011 × 1.2) respectively; and together .015; in all we have .114. The propelling force of .029 is slightly less than the .032 of §5, and the power required not appreciably less, so that there has been a slight loss of efficiency.

17. If we consider the power required under the conditions of the previous paper we find, corresponding to §12, that during the down-beats work is done at a rate

$$.5\rho V^3 k\,(1 + .09\,r^2/l^2)\,(L_1 + D_1 kr/l)\,r/l$$

and during the up-beats at a rate

$$.5\rho V^3 k\,(1 + .09\,r^2/l^2)\,(-L_2 + D_2 kr/l)\,r/l.$$

With the values $\beta = 12°$ throughout the wing on the downward stroke, and $\gamma = 0,\ 6,\ 12°$ for the three segments on the upward stroke, we find the power necessary for the down-beats is .090 and for the up-beats −.024. If the algebraic sum of these is permitted, as it might be in a mechanical model by the use of elastic devices, it would be .066; but in a bird the work that theoretically might be derived from the air on the upper beat would be mostly wasted; and as a reference to §13 will show, the obvious way to secure a positive expenditure of power is to make $L_2$ large and negative in the outer portion of the wing.

18. The question naturally arises how the efficiency of flapping would compare with that of a propeller used to drive the bird, with the wings fixed for gliding flight. With $LS\rho V^2$ to support the weight of the bird, which is $.52\,S\rho V^2$ for the rook, and the same angle $\beta$ for the whole of the wing we must have $L = .52$; so with a high lift wing we need $\beta = 4°$ and $D = .044$. Thus under the conditions of §10 a screw driving the bird would have to provide a resultant propulsion of .043 after providing .044 as the resistance of the wings; and the total thrust required would be $.087\,\rho SV^2$, the rate at which work was done being $.087\,\rho SV^3$. Unless therefore the efficiency of the propeller was as low as 87/139, or 63 per cent., the power required to drive it would compare favourably with $.139\,\rho SV^3$, the power expended in flapping the wings. If we were to use high velocity wings to support the bird when propelled by a screw, corresponding to $L = .52\ \beta$ would be 12° and $D = .046$; so the power would be almost the same as for high lift wings.

This comparison seems at first sight scarcely fair to the screw, for by giving the bird larger wings or a larger velocity we could secure a smaller angle of incidence with a bigger ratio of lift to drift, such as 16.6 with $\beta = 3°$ and high lift wings. In that case corresponding to an increased wing of area $S' = 2.6\,S$ the resistance would be $.031\rho SV^2$ and the necessary power would fall to $.074\rho SV^3$ instead of $.089\rho SV^3$. But an enlarged wing surface or velocity would enable the flapping bird to be designed with smaller angles of incidence, and its efficiency would undergo in all probability a decidedly greater improvement than 15 in 89.

On the whole in view of the complex nature of the actual movement of a bird's wing it seems likely that its efficiency would, if we could work it out in detail, compare not unfavourably with that of propulsion by a screw.

19. When comparing the square or round ended wings of birds of prey with the long pointed wings of a seagull or albatross the value of the former type in steep uphill gliding on vertical currents was indicated in a former paper.*

Over the ocean vertical convection currents must be extremely rare and non-flapping flight is probably explained in terms of variations in wind velocity

* "Meteorology and the Non-Flapping Flight of Tropical Birds." *Camb. Phil. Proc.*, XXI., p. 371, 1923.

due to turbulence or eddy motion. Evidence of the adequacy of this inland was given in the paper just referred to.†

A preliminary study of cinema films indicates that, as we have assumed, during a period when the bird is making a strong propulsive effort the upstroke is more rapid than the downstroke; but while the inner portion of the wing moves up fairly uniformly, the outer portion moves upwards with a "flick." During the first half of the up-beat the angular motion in space of this outer portion is very slow indeed, and the wing bends at the carpal joint; and during the second half the outer portion rapidly flicks upwards till the wing is straight again. As this paper has brought out, the support provided by the inner portion of the wing is exerted during a rate of moderate up-beat; but the propulsion provided by the tip is increased by rapid uplift and the bird secures the increase by the flick, which in a cinema picture strongly recalls the crack of a whip.

20. When comparing with photographs of birds in flight the angles of incidence of the outer portions of the wing, for example in §10 $+14°$ during a down-beat and $-10°$ during an up-beat, it must be remembered that these are relative to the actual motion of the wing through the air. In the first case the wing tip is moving at an angle of 23° downwards, so that its plane is inclined at 9° downwards; and during an up-beat the tip climbs up an angle of 32° so that the plane of the outer portion of the wing slopes upwards at 22°. It is obviously desirable to compare these angles with reliable measurements from cinema films of birds in actual flight.

### *Summary*

The analysis in the previous paper in this journal showed that an artificial bird of the dimensions of a rook with wings of known cross section flapped rhythmically would maintain itself in the air when travelling at the ordinary velocity. There is, however, energy wasted on the up-beat and no indication of ability to fly uphill or gain speed rapidly; and in the present paper it is shown that this may be secured by making the up-beats more rapid than the down-beats and by further twisting of the outer portion of the wings until its plane is inclined 10° below that of motion. In this way, as shown in §10, a climb up a slope of about 1 in 12 may be secured. The effect of making the wings narrower at the tip and wider at the base has also been considered; the effect is to increase the support and diminish propulsion so that more warping of the wing tips and more rapid beating would be needed. Cinema films show that when some sea birds make an effort to gain pace rapidly they raise their wings with a "flick"; so that while the inner portion moves uniformly up, the outer portion, after a short period of comparatively slow motion. is raised with considerable rapidity. The bird is thus enabled to utilise the large muscles that it possesses for raising the wing and gains in ability to accelerate rapidly. The rate at which work is done in flapping has also been investigated; it appears that the power expended when, under the conditions of the former paper, the bird is merely maintaining its horizontal motion will compare not unfavourably with that of a screw propeller maintaining the velocity with the wings outstretched in the gliding position. But when the bird is accelerating, as in §10, a screw with moderate efficiency would perhaps be the better.

† *Loc. cit.*, §§ 8–10, pp. 366-8.

# THE ARTICULATED CONNECTING ROD.

## SOME OBSERVATIONS ON THE PAPERS OF MESSRS FEARN AND FARREN

BY CAPTAIN J. MORRIS, B.A., A.F.R.AE.S.

The method adopted by Mr. Fearn in dealing with the above subject in the February number of the Journal is open to many serious objections. To begin with, it is at best approximate and as Mr. Farren points out in his article in the March number, Mr. Fearn's approximation, although near enough for displacements, give on double differentiation less close approximation for acceleration. And for less regular motions than in the case under discussion the approximation to the acceleration by Mr. Fearn's method might be comparatively poor.

The greatest objection which the writer has to Mr. Fearn's method is that it is far too intricate and cumbersome. It unnecessarily involves mathematics of a high order when the problem can be solved by means within the compass of any first year student of engineering.

Mr. Farren's method, on the other hand, is of extreme simplicity and as a graphical method could hardly be improved upon. But there is a very simple analytical solution and one moreover of the familiar type for the ordinary non-articulated case.

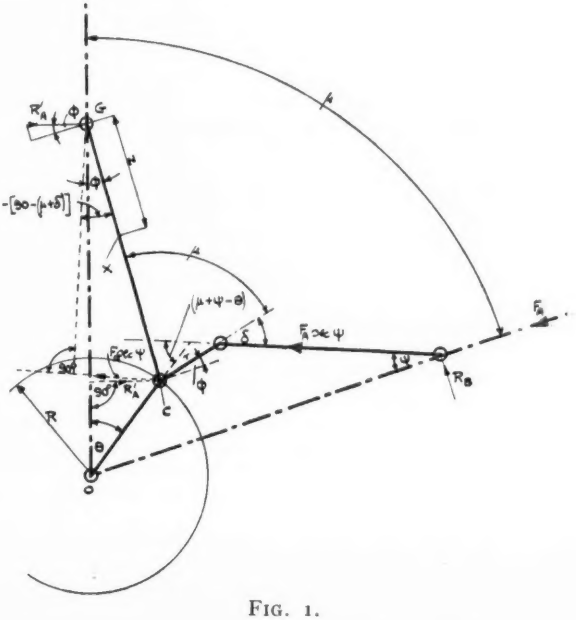



FIG. 1.

Consider Fig. 1 above, taken from Mr. Fearn's article.

We have.

$$L \sin \phi = R \sin \theta \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$l \sin \psi = R \sin (\mu - \theta) + r \sin \phi \quad . \quad . \quad . \quad . \quad (2)$$

$$\rho = R \cos (\mu - \theta) + r \cos \phi + l \cos \psi \quad . \quad . \quad . \quad . \quad (3)$$

Hence, using (1) and (2)

$$\rho = R \cos (\mu - \theta) + r \{ 1 - (R^2/L^2) \sin^2 \theta \}^{\frac{1}{2}} + l [1 - (R^2/L^2) \{ \sin (\mu - \theta) + (r/l) \sin \theta \}^2]^{\frac{1}{2}} \quad . \quad . \quad . \quad (4)$$

or

$$\rho = R \cos (\mu - \theta) + r \{ 1 - (R^2/L^2) \sin^2 \theta \}^{\frac{1}{2}} + l [1 - (R^2/l^2) (1 - 2 r \cos \mu/L + r^2/L^2) \sin^2 (\theta + \epsilon)]^{\frac{1}{2}} \quad . \quad . \quad (5)$$

where

$$\tan \epsilon = (\sin \mu)/(r/L - \cos \mu)$$

In the special case where $\mu = 90^\circ$ (5) becomes

$$\rho = R \sin \theta + r [1 - (R^2/L^2) \sin^2 \theta]^{\frac{1}{2}} + l [1 - (R^2/l^2) (1 - r^2/L^2) \sin^2 (\theta + \epsilon)]^{\frac{1}{2}} \quad . \quad . \quad . \quad (6)$$

where $\tan \epsilon = L/r$.

All we require to know for (5) is the form of the expansion of an expression such as

$$(1 - q^2 \sin^2 \theta)^{\frac{1}{2}}$$

where $q$ is less than unity.

This expression is the ordinary one which arises in the non-articulated case. It can be found in any text-books on balancing of engines. The effect of the articulation is readily observed by comparing the expansion of (5) with the case when $r = 0$.

The above observations were made by the writer when he presided over the reading of the paper by Mr. Fearn on the 10th February last.

University of London Club,
21, Gower Street, W.C.1,
2nd March, 1927.

Dear Sir,—There is still a further solution to the problem of the articulated rod which may be obtained by the direct expansion of equation (4) for $\rho$. This gives an infinite series of the form

$$\rho = \alpha_0 + \alpha_1 \cos \theta + \alpha_2 \cos 2\theta + \alpha_4 \cos 4\theta + \ldots$$
$$\beta_1 \sin \theta + \beta_2 \sin 2\theta + \beta_4 \sin 4\theta + \ldots \text{ etc.},$$

where $\alpha$ and $\beta$ are themselves infinite convergent series and functions of the known lengths of the component parts of the linkwork. This method was given, with considerable detail, by Captain J. Morris at the lecture on the Articulated Rod.

The writer had previously used this method and found that, for the same degree of accuracy, a greater number of harmonics were required than with the approximate method—the reason for this being obvious.

Mr. Farren's elegant method (given on page 234) overcomes the difficulty of the double differentiation of the displacement curve and provides a procedure for those who prefer graphical methods. Of course. one must be extremely wary when analysing a displacement into the harmonics of a Fourier's series, since the differentiated series may give but a poor approximation to the velocity if there be a sudden change in curvature.

It is in such cases, and especially when considering overhead values operated by a mechanism with many sliding and turning pairs, that Mr. Farren's ingenious suggestion is most useful—that is, to replace the motion by equivalent four-bar mechanisms.

Yours faithfully,
E. J. FEARN.

# THE TRAINING OF AIRCRAFT APPRENTICES

BY WING COMMANDER C. D. BREESE, A.F.C.

[*The following is a revise of Colonel the Master of Sempill's remarks at Wing Commander Breese's lecture. By an error the Chairman's rough notes were published last month.*]

Before asking the Lecturer to reply, I should like to ask him one or two questions.

What percentage of the mechanics now in the R.A.F. have been trained at Halton?

It would appear to me that two classes of mechanics are wanted. Firstly, those who are competent to do what might be termed running repairs; and secondly, those who are highly skilled and capable of undertaking major repairs generally. The scheme in force at present seems to fill neither of these requirements. General education is of course needed, but far too much attention would seem to be paid to this subject. The aim is surely to train out personnel who will be able to keep the equipment of the R.A.F. in efficient condition and this aim should not be lost sight of. It is presumed that but a small number of those trained will be able to get Commissions, and this being so, the present system would seem liable to engender dissatisfaction. Reference is made to the need of preventing boredom in practical work, but I would suggest that if the proper kind of work is provided and if those under training feel they are doing work of practical value, this itself will prevent what is now feared.

Much has been said about the excessive amount of drill and it would be interesting to know how much time is spent in this particular form of activity?

There seems to be a difficulty in obtaining examples of modern engines. This should not be so. It will be well, too, to bear in mind that the stripping and erecting of engines kept for that purpose alone can be carried too far. Work done on scrap engine parts and the making of articles serving no useful purpose in themselves is much to be deprecated. No system of this nature can be a success and it is furthermore a slight to their skill, which is undoubtedly very high, judging from the articles we see before us that have been made at Halton.

As compared to the training of boys for the Navy there would seem to be far too short a time spent in practical work. The naval training lasts five years and the Air Force three. In the former there are 47 hours per week practical training over the period indicated and in the latter only 16.

I understand that the Trades Unions will not accept ex-R.A.F. apprentices although they will similar hands from the Navy. Why is this and how is it to be remedied?

It would be interesting to know whether the young R.A.F. officers of to-day take a real interest in their mechanics and their work?

Taking again the question of practical work, the building of aeroplanes from scrap materials should be stopped forthwith. Most certainly aeroplanes should be built, but they should be built with the intention of being flown. Imagine the difference this would make to the outlook of the boys.

There can be no doubt that these and all kindred questions touch on the fringe of the vitally important question of the Technical Organisation of the R.A.F. To-day this can surely not have reached its final form. Changes will be made and perhaps are imminent. The creation of an Engineering Branch cannot be long delayed and in fact the need for such a step is being realised to an increasing extent. The solution of these problems that arise out of this lecture will only be effectively dealt with when an engineer-in-chief, or the equivalent, takes a seat on the Air Council.

## CORRESPONDENCE

The Prudential Insurance Company of America.
Home Office: Newark, New Jersey,
January 5th, 1927.

COLONEL W. SEMPILL, *Chairman, Royal Aeronautical Society.*

Dear Mr. Sempill,—I read with much interest your letter to the *Times* of December 15th with reference to accidents in the R.A.F. You are entirely correct in your point of view regarding the urgency of more complete and trustworthy statistics and particularly as regards the ratio of fatalities to flying hours. I have for several years been engaged in the study of aviation fatalities in the United States and Canada through the co-operation of our military and naval authorities and the Royal Air Force of Canada. I have also been concerned with accidents in commercial aviation at home and abroad.

It has occurred to me that possibly you might be willing to extend to me the privilege of an interview when I shall be in England this summer during the latter part of June to attend the International Actuarial Congress. I expect to have letters of introduction from General Patrick and Assistant Secretary MacCracken, as well as Admiral Moffett, to the local authorities in England, but perhaps you can assist me in my efforts to obtain a clearer understanding of the British situation. I am sure you will agree with me that as long as insurance companies are extremely reluctant to accept risks on pilots and mechanics, commercial aviation cannot be expected to make the progress anticipated. It seems to me of the very first importance that the public should arrive at a better understanding of the true nature of the hazards, which individually considered, are under safe conditions of flying practically negligible. We fly in this country over a million miles to a fatality in the Air Mail Service and even in the Army and Navy the risk is by no means as serious, as a matter of fact, as is generally assumed to be the case. The greatest difficulty in dealing with the subject statistically rests upon the small number of men concerned. If we had one hundred times the flying operations which we have at present, it goes without saying that the actual number of fatalities would probably not be more than ten times what it is at present. In other words, the risk factor is conditioned by the number of operations. The more extended the operations the less will be the true ratio of fatalities.

It is regrettable that the British Government should never have published data corresponding to our own military and naval statistics. All of these are available in published form.

Anticipating the pleasure of making your personal acquaintance, I remain,
Very truly yours,

FREDERICK L. HOFFMAN.

*To the Editor of the* JOURNAL OF THE ROYAL AERONAUTICAL SOCIETY.

Dear Sir,—We note that in the February issue of your Journal, you published on page 166 an extract from " La Revue Petrolifiere " in regard to lubricating oils.

In this extract various brands of lubricating oil are mentioned, but we regret

to note that it is not complete, as a mention of our Castrol " R " exists in the said report and reads as follows:—

> " Compound Oils.—To this type belongs the oil ' Castrol ' R comprising above all castor oil, effectively polymerised by a thermal treatment and deprived of its acid content, with a small percentage of other oils. This oil resists high temperatures better than castor oil, and it is also the oil employed in most of the big events."—(*Translation.*)

We feel that in printing the portion that you have, and omitting the above reference to Castrol " R," you have not treated us quite fairly.

Yours faithfully,

C. C. Wakefield & Co., Ltd.,
W. R. Graham,
*Managing Director.*

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## REVIEW

### *Mechanics Applied to Engineering*

John Goodman, Wh.Sch., M.Inst.C.E. Longmans, 2 vols. 14/6 and 16/- net respectively.

It is not surprising that the ninth edition of *Mechanics Applied to Engineering* has been called for. Professor Goodman's book was first published over thirty years ago and is one specially suitable for engineers and students who already possess a working knowledge of elementary mathematics and theoretical mechanics. It is one of the most practical and useful books on the subject published, and can be thoroughly recommended.

To the original single volume of his work, Professor Goodman has now added a volume consisting chiefly of worked-out examples, the arrangement of the chapters following the original work. This volume should prove of the utmost use to the practical engineer. Many hundreds of examples are fully worked out, with references to Volume I. where there may be any difficulty in following the argument.

## BRANCH NOTICES

### YEOVIL BRANCH

## TESTING OF AIRCRAFT MATERIALS

*Paper read at the 9th Meeting for prospective Ground Engineers, held at the Westland Aircraft Works, Yeovil, on the 5th January, 1927,*

BY W. SUTCLIFFE, ESQ., M.SC.

(OF THE WESTLAND AIRCRAFT WORKS, YEOVIL).

In no other branch of engineering is an accurate knowledge of the strength of materials so important as it is in aeronautics, where the weight of every part has to be reduced to the minimum compatible with safety.

Mechanical tests have to be resorted to at every stage, first to determine the qualities of the material and secondly to check the strength of each part.

In the former section of testing falls the ordinary routine check testing to determine the reliability of a batch of material and also to determine whether the heat treatments, where these are necessary to give the requisite strength to the material, have been successfully carried out. It is this section only that will be dealt with in this paper.

The second section of testing covers the testing of wiring lugs, ribs, spars and sometimes the destruction test of a complete machine to determine their respective strengths.

The materials used in aircraft construction are very diverse, but may be classified generally under metals and timber.

Considering primarily the testing of metals. For this purpose it is evident that some mechanical device is essential, and the one most commonly employed is the ordinary tension and compression machine. With the aid of this machine the loads that specimens will sustain in tension and compression can be accurately determined. Other forms of apparatus are also used for obtaining information on the characteristics of the materials which cannot be obtained by an ordinary tension test. These include the Brinell Hardness Tester, the Shore Schleroscope, the Izod Impact machine, Torsion Tester, and machines for determining alternating fatigue stresses.

In these Westland Works the tests are restricted to the ordinary tension and compression tests, although Brinell tests can be carried out between the compression pallets of the machine.

It is an established fact that the shape of the test specimen has a very pronounced effect upon the results. Therefore in order to make all results comparative, the British Engineering Standards Association have devised a standard form of test specimen which is described in their Specification A.4.

There are factors, however, which preclude the use of these large test pieces, as for example, the capacity of the testing machine and also the dimensions of the material from which the specimen can be cut. These difficulties are overcome by the fact that smaller test pieces having the same geometrical proportions will give comparable results to those obtained with the larger specimens.

It is of the utmost importance to see that the specimens are correctly machined and that the surface is quite free from scratches and tool marks. These faults give rise to very misleading results owing to a concentration of stress being produced where they occur, thus causing the specimen to fracture at a much lower value than the true one.

Another point to be noted is that the more gradual the curvature at the shoulders the less the possibility of the fracture occurring where the radius meets the parallel portion of the specimen.

The question now arises as to the method of securing the test pieces in the machine. It is of paramount importance that the load shall be applied axially to the specimen otherwise secondary bending effects are induced, which cause the specimen to fail at a lower observed value than the true one.

The method of gripping sheet and small diameter specimens is by means of wedge grips. These are small hardened wedges which fit into tapered slides in the machine heads. The faces which come into contact with the specimen are serrated. The specimen is placed between these two wedges and a small thumb-screw forces the wedges into the tapered slides. This gives an initial grip on the specimen which is sufficient until a considerable load is exerted by the machine, when the tension in the specimen automatically tends to pull the wedges still further into the slides, thus increasing their grip.

For tubes up to ½in. dia. the ends are plugged where the wedges come in contact, but above this size strips have to be cut from the tube and these are then treated as ordinary sheet material.

On some machines special forms of shackles are employed for specimens of large diameter and the ends are machined to fit them.

In all test pieces the ends should be machined so that they will be concentric with the centre portion.

The method of holding cables should be noted. A length of cable, not less than seven lays in length, has the ends frayed out and preferably the wires hooked back a little. A tapered lead blob is cast on each end, tapered to fit the shackles of the machine.

The results generally required in mechanical testing are:—

(1) Elastic limit.
(2) Yield stress.
(3) Ultimate stress.
(4) Elongation.
(5) Reduction of area.

An explanation of the terms commonly employed in this subject may be helpful here.

### *Load*

This term signifies the external force which is applied to the specimen and is measured in pounds or tons.

### *Stress*

The stress in the specimen is the internal or molecular force called into play on the application of external forces and is expressed as the load divided by the cross sectional area, the amount thus being expressed in pounds or tons per square inch.

### *Strain*

When a state of stress exists in a body, distortions or alterations in shape take place. These are referred to as strains.

### *The Elastic Limit*

Is scientifically defined as the maximum stress to which the material may be subjected, such that on the removal of this stress the material will return to its original shape and dimensions. The B.E.S.A. definition, however, is somewhat different and it is defined as that point at which the extensions cease to be proportional to the loads. In a stress-strain diagram, plotted to a large scale, it is the point where the diagram ceases to be a straight line and become curved. This definition is analogous to the scientific limit of proportionality. When a material is subjected to this stress and the stress is then removed, the whole of the strain will not disappear, that is, the specimen will have a slight permanent set. For the determination of these stresses very delicate instruments are required and a highly skilled operator in their use is essential.

### *Yield Point*

This is the stress at which the elongations increase at a very much greater rate than the increase in load. In a number of materials this point is well defined. The B.E.S.A. defines the yield point as the stress where the extension of a specimen increases without increase of load. The practical interpretation is the stress at which a distinctly visible increase occurs in the distance between the gauge points on the test piece observed by using dividers; or at which, when the load is applied at a moderately fast rate there is a distinct drop of the testing machine lever. A steel test piece at the yield point takes rapidly a large increase in extension amounting to more than 1/200 of the gauge length.

### *Proof Stress*

For a material which has no definite yield point (such as the light aluminium alloys) an arbitrary stress has been determined to which the material is subjected for 15 seconds. On the removal of this so-called proof stress the distance between the gauge points is measured. No permanent set shall be indicated neglecting that less than $\frac{1}{2}$ per cent. of the distance between the gauge points.

### *The Ultimate Stress*

This is the stress obtained by dividing the maximum load which the material sustains by the original cross-sectional area. It is most essential to note that the original cross-sectional area is used. This value is the one which is always employed in the specifications.

### *Elongation*

This term is self-explanatory and denotes the amount the material has stretched. After fracture the two portions are placed together and the distance between the gauge points is measured. In ordinary works testing a pair of dividers is used for measuring these lengths as only an approximate idea is required. The elongation is given as a percentage of the original length.

### *Reductions of Area*

When a tensile specimen is pulled the extensions produced are primarily distributed over the whole length, but after a certain stress is reached (the ultimate stress) the extensions become localised and a waist is formed in the test specimen. It is at this waist that fracture finally occurs. The difference between the original cross-sectional area and the final cross-sectional area is known as the reduction of area, and is expressed as a percentage of the original area.

A few typical examples of the mechanical tests called for in the specifications are shown below.

### *Specification B.13*

This is for brass for high speed screwing or turning and has an analysis of

Copper not less than 58 per cent.
Lead not more than 2 per cent.
Zinc the remainder.

A tensile test on a standard test piece must give the following results:—

Ultimate stress not less than 20 tons per sq. inch.
Yield point stress not less than 8 tons per sq. inch.
Elongation not less than 12 per cent.

A bending test is also taken. Bend test pieces are to be taken from bars of $\frac{3}{4}$in. dia. (or width across flats) or under and tested full size as rolled, drawn, or extended. For bars above $\frac{3}{4}$in. dia. the test piece is to be turned down to a dia. of $\frac{3}{4}$in. The test piece must withstand being bent to an included angle of 75° with an internal radius equal to twice the diameter of the test piece without developing any cracks.

### *Specification 2L1*

WROUGHT LIGHT ALUMINIUM ALLOY BAR (DURALUMIN).

Tensile test. Results from a standard test piece.

| | Bars above $2\frac{5}{8}$in. dia. (Hammered) Not less than | Bars up to $2\frac{5}{8}$in. dia. (Extruded or rolled) Not less than |
|---|---|---|
| Ultimate tensile stress ... | 17 tons per sq. in. | 25 tons per sq. in. |
| Proof stress ... ... | — | 13·5 ,, ,, |
| Elongation ... ... | 15 per cent. | 15 per cent. |
| Reduction of area ... ... | 18 per cent. | 20 per cent. |

### *Specification 2L3*

WROUGHT LIGHT ALUMINIUM ALLOY SHEET (DURALUMIN).

Tensile test. Ultimate stress (not less than) 25 tons per sq. in.
Proof stress (not less than) 13.5 tons per sq. in.
Elongation (not less than) 8% for sheets below 25 S.W.G. (.02in.).
12% for sheets 25 to 18 S.W.G. (.02in.—.048in.).
15% for sheets 18 S.W.G. and above.

### *Specification 2S1, Part 1*

MEDIUM CARBON STEEL BARS.

A tensile test on a standard test piece shall give the following results:—

| Diameter. | $\frac{5}{16}$" and under. | Over $\frac{5}{16}$" and under $1\frac{1}{2}$". | $1\frac{1}{2}$" and over. |
|---|---|---|---|
| Ultimate stress (between) ... | 35-42 tons | 35-42 tons | 35-42 tons |
| Elongation (not less than) ... | per sq. in. | per sq. in. | per sq. in. |
| Reduction in area (not less than) | 12% | 15% | 15% |
| Elongation plus reduction in area | 35% | 40% | — |
| (not less than) ... ... ... | — | — | 55% |

*Bending Test.*—Test pieces shall be taken from bars of $\frac{3}{4}$in. dia. or under and tested full size as rolled or drawn. For bars above $\frac{3}{4}$in. dia. the bars shall be turned down to $\frac{5}{8}$in dia. The test piece shall withstand bending through an angle of 180° without showing signs of failure, the internal radius of the bend being equal to the diameter of the bar.

### *Specification S.3*

MILD STEEL SHEETS (FOR WELDING).

*Tensile Test*—A standard test piece when tested in tension must show not less than—

Ultimate stress 26 tons per sq. in.
Yield point 18 tons per sq. in.

*Bending Tests.—Close Bend.*—The corner of every sheet is to be bent over and closed down on a sheet of the same thickness. The sheets must stand this treatment without showing any signs of failure.

*Reverse Bend.*—A strip $1\frac{1}{4}$in. wide, filed or machined, to have smooth edges and rounded corners, is to be bent backwards and forwards in the following way: The strip is to be fixed in a vice which has the edges of the jaws rounded to a radius equal to three times the thickness of the sheet. The projecting end of the strip is then to be bent at right angles to the fixed part, first to one side then to the other, for a number of times till it breaks. The strip must stand without cracking three such reversals; the first through 90° is not counted.

### *Specification 2S20*

TINNED STEEL SHEETS.

*Bending Test.*—The corner of every sheet is to be bent over and closed down tight on the sheet. The sheets must stand this without showing any signs of cracking.

*Dishing Test.*—Sheets are to be dished with a hemispherical punch and die (which has a radius of approximately $1\frac{1}{4}$in.) so as to have an indentation formed in them at least $\frac{3}{4}$in. deep. The dishing is to be done by blows of a hammer and not with a press. The material should stand this treatment without showing signs of cracking.

### *Specification T.1*

35-TON CARBON STEEL TUBES.

*Tension and Compression Tests.*—Test pieces shall give the following results without further heat treatment or other manipulation:—

Ultimate stress, in tension, not less than 35 tons per sq. in.
Yield point, in tension, not less than 30 tons per. sq. in.
Yield point, in compression, not less than 30 tons per sq. in.

*Softening Test.*—This test is intended to prove that the tubes will not be unduly soft after they have been annealed, welded, brazed, or otherwise heated. A test piece is heated to a full red heat at one end for at least five minutes while the other end remains cold and is then to be allowed to cool freely in the air. The sample so treated must give the following results when tested in tension:—

Ultimate stress not less than 28 tons per sq. in.
Yield point not less than 18 tons per sq. in.

*Flattening Test.*—The tube is to be flattened at the end or at any point where defective material is suspected by a few blows (not more than six), till the sides are not more than three times the thickness of the metal apart. The tubes must stand this without cracking.

*Crushing Test.*—Samples of tube are to be crushed endwise until the outside diameter is increased in one zone by 25 per cent. or until one complete fold is formed. The samples must stand this treatment without cracking.

### *Specification T.5*

50-TON CARBON STEEL TUBES.

*Tension and Compression Tests.*—Normalised tubes without further heat treatment or other manipulation must give the following results:—

Ultimate stress, in tension (not less than) 50 tons/sq. in.
Yield point, in tension (not less than) 45 tons/sq. in.
Yield point, in compression (not less than) 45 tons/sq. in.

*Softening Test.*—One end to be heated to a full red heat while the other end remains cold and then allowed to cool in air. When tested the ultimate stress is to be not less than 30 tons per sq. in. Yield point is to be not less than 18 tons per sq. in.

*Flattening Test.*—A sample of the tube of length equal to its own diameter is to be flattened till the sides are not more than eight times the thickness of the metal apart. The samples must stand this treatment without cracking.

### *Specification T.26*

MILD STEEL TUBES.

*Tensile Test.*—An ultimate stress of 20 tons per sq. in. and a yield point of 11 tons per sq. inch are required under this specification.

*Flattening Test.*—The tube is to be flattened at the end or at any point where defective material is suspected by a few blows (not more than six) till the sides are more than three times the thickness of the metal apart if the tubes are half hard. Annealed tubes are to be flattened close. The tubes must stand this treatment without cracking.

*Crushing Test.*—Samples of tube are to be crushed endwise until the outside diameter is increased in one zone by 25 per cent. or until one complete is formed. The specimen shall not crack.

*Bending Test.*—Tubes of less than $\frac{3}{4}$in. diameter and all trailing edge tubes must stand being bent through 90° round a radius not greater than 10 diameters without serious deformation in section or signs of failure.

Having dealt briefly with the testing of metals, attention can now be directed to the testing of timber. The strength of a piece of timber is greatly different in different directions, being much greater for tension and compression along the grain than across it, in which direction the fibres have not to be broken but merely torn from one another, the resistance being more a question of adhesion than of strength in the usual sense. Further, the strength of a piece of timber depends upon the part of the tree from which it is cut, whether from the heart-wood or the sapwood and the upper or lower part of the tree. The strength is also greatly affected by the amount and method of seasoning it has undergone, the place and soil on which it was grown, the age of the tree and the season at which it was cut down.

Discussing primarily the testing of Sitka (silver) spruce as this is the most commonly used wood in aircraft construction.

The standards to which all aircraft spruce must conform are given in D.T.D. Specification No. 36, and these will be appended to the description of their respective tests.

The tests required are as follows, the determination of—

(1) Density.
(2) Moisture content.
(3) Brittleness.
(4) Ultimate compressive strength.
(5) Modulus of elasticity.
(6) Modulus of rupture.

Occasionally a splitting test is taken to determine the straightness of the grain and fibre, though this can usually be determined visually in this type of timber.

It is the usual procedure at these works to take the brittleness test first. This is done to obviate any further work or tests on material that is unsatisfactory in this respect. The test consists of taking a piece of timber about 6in. long by 1¼in. by 1in. section from the end of a plank and making a saw cut on the inch side a ¼in. deep, so that there will be a cross-sectional area of one inch at this point. One end is fixed in a vice and a blow with a heavy hammer is struck at the free end, thus fracturing the test piece. The fracture is then examined and if there are some fibrous splinters showing, the timber is considered to be OK. Should the fracture show a "clean break," that is no indication of a fibrous nature, the timber is not considered satisfactory. This latter type of fracture is spoken of as being "carroty." In the case of a plank showing this defect it is usual to take another test from the other end, as it is often found that a plank may be brittle at one end and yet give an exceptionally good fibrous fracture at the other, due to the wide variations which occur in timber. A portion of the plank therefore can be utilised. The brittleness specimens have to be stored for reference purposes.

The next test is the determination of density. The plank has now been planed up and it is then measured and weighed. The weight being divided by the cubical dimension gives the weight per cubic foot. This value is the density of the timber and must not be less than 25 lbs. per cubic foot when the timber has a moisture content of 15 per cent.

The moisture content test is carried out as follows:—A sample of the timber is split up into small sticks about the size of match sticks. These are weighed immediately and then placed into the drying oven. The temperature of the oven should be 100°C. (212°F.). The most convenient oven for this purpose is one that is surrounded by boiling water. The specimens are left in the oven until on weighing it is found that the weight has become constant. They are then removed finally from the oven and weighed immediately.

If it is assumed that the final weight was 100 units and the initial weight was 115 units then 15 units of moisture have been driven off. The moisture content is given by

$$\{ (\text{Initial wt.} - \text{Final wt.})/\text{Final wt.} \} \times = 100 \{ (115 - 100)/100 \} \times 100 = 15\%.$$

It is seen, therefore, that the moisture content is the percentage of moisture calculated on the dry weight of the timber.

The limits of moisture content are from 14 per cent. to 17 per cent. It is necessary to take a moisture test on every tenth plank for the requirements of this specification and also for all timber which has a density between 25 lbs. per cubic foot and 26.5 lbs. per cubic foot.

A compression test is next taken. The specimen being about 3in. overall length and about 1⅛in. diameter in the central portion which is parallel for a length of 1¼in. The ends are 1½in. diameter. After the diameter has been accurately ascertained the specimen is placed between the pallets of the testing machine and the load applied. The rate of loading has to be between the rates of 3,000 and 6,000 lbs./minute and great care must be exercised to stop loading as soon as the beam begins to fall. From the load and diameter the compression stress in lbs./sq. inch may be calculated and this will have to be corrected if the moisture content differs from the standard of 15 per cent. The minimum compression stress allowed is 4,500 lbs. per sq. inch at 15 per cent. moisture. If the moisture content is greater than 15 per cent. then the experimental values shall be increased at the rate of 200 lbs. per sq. inch for each 1 per cent. variation of moisture and vice versa.

The modulus of elasticity and the modulus of rupture are determined from a beam test. A specimen is cut approximately 40 in. long by 2in. by 1in. section. This beam is subjected to bending such that the 2in. is the depth of the beam and the 1in. is the width. The two point method of loading is employed in order that pure bending can be obtained during the test. The loads are applied through roller bearings and shackles, at the neutral axis of the beam. Between the two inner loading points no shearing action occurs and the deflection can be measured which will be independent of any deflection due to lateral shear. The load is applied in increments of 100 lbs. and the deflection for each increment is noted. This is continued until a compression shake appears in the beam specimen and the beam specimen and the deflectometer is then removed. The load is then continuously applied until fracture takes place; the final load being noted. A curve is plotted with deflections as ordinates and loads as abscissæ. It is found that up to a certain point just below the load at the appearance of the compression shake, the curve is a straight line. From this portion of the curve is determined the modulus of elasticity of the timber, or as it is sometimes referred to—Young's modulus. It is commonly represented by the letter "E."

"E" may be defined as the intensity of stress per unit strain, *i.e.*,

$$\text{Stress intensity} = \text{Strain} \times \text{"E."}$$

As strain is non-dimensional "E" is measured in the same units as stress intensity, usually lbs. or tons per sq. inch.

If $W_1$ and $W_2$ are two loads on the straight portion of the graph and their respective deflections are $d_1$ and $d_2$, then

$$E = (W_1 - W_2)\, al^2 / 16\, I\, (d_1 - d_2)$$

where $a$ is the distance between the outer point of support and the adjacent inner loading point; $l$ is the length of the neutral axis at the centre of which the deflection is measured.

$I$ = moment of inertia, or as it is sometimes termed the second moment of the cross section of the beam. Its value is given by $I = bh^3/12$, where $b$ is the breadth of the beam and $h$ is the depth of the beam.

As the size of the beam and the mode of loading and measuring of deflection is always the same the formula becomes

$$E = 144 \times (W_1 - W_2)/(d_1 - d_2)$$

where $W$ is in lbs. and $d$ in inches.

The minimum value of $E$ that is allowed is 1,500,000 lbs. per sq. inch at 15 per cent. moisture. The experimental values shall be increased if the moisture content is above 15 per cent. at the rate of 25,000 lbs. per sq. inch for each 1 per cent. variation and vice versa for moisture contents below 15 per cent.

The modulus of rupture is determined from the breaking load. If the conditions of elasticity held good up to rupture, then the modulus of rupture would be the fibre stress at the outer layers of the test specimen, but as these conditions do not hold good the value which is derived is not a true intensity of stress. However, it serves as a very useful comparison, hence its use. The value is derived from the formula

$$\text{Modulus of rupture} = 3Wa/bh^2$$

where $W$ is the total breaking load, $a$, $b$ and $h$ being as mentioned in the previous calculations.

The minimum value of the modulus of rupture is 8,000 lbs. per sq. inch at 15 per cent. moisture and these experimental values are increased for decreasing moisture content and decreased for increasing moisture content at the rate of 370 lbs. per sq. inch for each 1 per cent. variation of moisture content.

Other timbers are tested in a similar manner as to that indicated in the foregoing.

## COVENTRY BRANCH

The first lecture of the season was given by Squadron Leader Sir Quentin Brand, on Thursday, February 24th, 1927, Mr. J. D. Siddeley presiding, the subject being "The Pioneer Flight to the Cape."

Sir Quentin Brand gave a very interesting account of his experiences and left in the minds of his hearers an extremely vivid impression of the tremendous difficulties which were met and overcome. Adopting a colloquial style, from first to last, he held the interest of a good sized audience, who appreciated his lecture to the full.

Owing to the advance made in aviation since those days, the hardships undergone were more severe, and it is all to the credit to those intrepid pioneers that they accomplished their aim.

The actual flying time was 107½ hours, the average speed about 72 m.p.h. The machine used was a Vickers-Vimy aeroplane with two Rolls-Royce "Eagle" engines, and additional accommodation for petrol. The longest spell of which the machine was capable was a thirteen or fourteen hours flight. The best actually done on the trip was one of eleven hours. There was a crew of four, and these with their personal luggage, rations and five cwts. of spares made it a fairly heavy laden machine.

They started from Brooklands via Paris to the Alps where they encountered a peculiar and pronounced drifting from their course, due to the deflection of the prevailing winds by the mountains. Thence by way of Genoa and Rome and down the west coast of Italy, after refilling with petrol and oil at the great aerodrome built at Rome for the Rome-Tokio air service. The "hop" across the Mediterranean was begun at 9 p.m. in the hope that more settled conditions would prevail than in day time, but this proved to be a false hope. Cyclonic weather was encountered and when the aeroplane crossed the coast of Africa it was far off its course to the westward.

After coasting eastwards and then south, the lighthouse at Alexandria showed up as a welcome landmark and the next stop was made at Cairo. From there to Khartoum was a thousand mile hop, and a further 650 miles brought them to Wadi Halfa, where one engine "konked" and the aeroplane crashed on landing, leaving the party no alternative but to return to Cairo by rail. There a new machine was forthcoming and the flight continued. The journey southward from this point was one of increasing interest and many fine photographs were secured on the way. These photographs are really astonishingly fine considering that they were taken with a Vest Pocket Kodak, and the enlargements used as slides to illustrate the lecture lent added interest to the lecturer's remarks. Those of the Victoria Falls were especially fine and convey in remarkable manner the grandeur of the world-famous scene.

The journey was not without its humorous interludes, many of these being supplied by the native labour recruited at the various stopping places.

Sir Quentin Brand paid a high tribute to the Armstrong-Siddeley "Puma" engine, which over a run of 2,800 miles gave no trouble whatever, and this was accomplished with nothing more in the way of spares than six sparking plugs.

Cape Town was reached 44 days after leaving Brooklands and so closed one of the finest chapters in the history of aviation.

In proposing a vote of thanks to the lecturer, Major Green compared the conditions of the flight with modern conditions and emphasised the fact that the many hardships encountered called for a very high degree of courage and resource.

The average speed of 72 m.p.h. was really a very fine achievement when one considers the conditions under which these results were achieved.